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The Seventy-Eighth Meeting of the American Astronomical Society

By CURVIN H. GINGRICH

Upon the invitation of Dr. J. Allen Hynek, director of the Emerson McMillin Observatory of Ohio State University, the seventy-eighth meeting of the American Astronomical Society was held on the campus of that University from December 28 to December 31, 1947. The dates for this meeting overlapped somewhat those for the meeting of the American Association for the Advancement of Science in Chicago. Consequently those members from the North and West whose route lay through Chicago took advantage of brief stop-overs in Chicago to attend sessions of Section D (Astronomy) or others of special interest. A session which attracted many astronomers, geologists, and others was held in the Grey Room of the Sherman Hotel on Saturday afternoon, December 27. This was a symposium on the topic "The Origin of the Earth." Dr. Forest Ray Moulton presided over this session and papers were presented and discussed by prominent geologists and astronomers. The room was filled to overflowing for this session. The address by Dr. James B. Conant, president of Harvard University, as retiring president of the Association on Saturday evening on the subject "The Role of Science in Our Unique Society" was another definite focal point of interest.

This schedule made the Sunday afternoon train from Chicago to Columbus the logical one for those astronomers bound for the Columbus meeting. The writer was among the hundreds of passengers waiting eagerly and impatiently to board that train. In looking about he saw familiar faces here and there in the crowd but only a nod of recognition was possible then. However, the astronomers succeeded quite well in congregating in one coach and for them the Columbus meeting began even before the train left Chicago.

The lateness of the arrival of the train in Columbus prevented this group from attending the first regularly scheduled event. It was a reception at the home of the President of the University, Dr. Howard L. Bevis. The fact that the President's home and the McMillan Observatory are situated side by side enabled the strangers to find their way readily. Astronomers, naturally, are skilled in recognizing observatories although they may not be so adept in identifying presi-

dents' homes. Being one of the late arrivals, the writer has only hearsay reports of this occasion. The consensus was uniformly very favorable and the event served to start the social features of the meeting at a high level.

The members and their guests were comfortably housed and dined in Neil Hall, a large dormitory for women on the campus. The large and well-furnished lobby quickly became a social center and between the several sessions was alive with motion, conversation, and good cheer.

There was no delay in beginning the regular sessions on Monday morning. Although a period of ten or fifteen minutes, depending upon temperature and temperament, was required to walk from Neil Hall to the auditorium in the Social Administration building where the sessions were held, practically all concerned were present at the scheduled hour of 9:00 o'clock. The president, Professor Otto Struve, called the meeting to order and, after a few remarks, introduced Professor N. Paul Hudson, Dean of the Graduate School of Ohio State University, who in the name of the University appropriately welcomed the American Astronomical Society to the campus. The reading and discussion of papers then became the order of the day. The session was terminated at 12:00 noon, for the purpose of securing the usual group photograph, with the result as shown in Plate I accompanying this paper.

The afternoon session was begun at 2:00 o'clock with a brief business period. The only item of business which was transacted was the adoption of the revised schedule of life-membership fees, which was printed in detail on the program. The secretary, Dr. C. M. Huffer, announced the list of names of newly elected members of the Society. This list, as augmented later, is given at the close of this report. The presentation of papers was then resumed and continued until five.

At 8:00 on Monday evening the members assembled in the auditorium to hear the second Henry Norris Russell Lecture given by Dr. Walter S. Adams, emeritus director of the Mount Wilson Observatory. Dr. Adams spoke on the subject "Gaseous Clouds in Interstellar Space." The large auditorium was filled to capacity for this occasion, the usual audience having been supplemented by a number of visitors from the University and from the community.

Tuesday was the day set aside for a visit to the Perkins Observatory at the invitation of the director, Dr. N. T. Bobrovnikoff. At 8:30 A.M., in three chartered buses and several private cars, the entire party was transported some twenty miles north to the appointed place. After a quick inspection of the spacious and well-furnished library, the rather luxurious offices, and the 69-inch reflector, the party assembled in the large lecture hall for the symposium papers. Dr. Struve called the session to order and introduced Dr. Bobrovnikoff who presided during the morning and afternoon sessions. The subject of the symposium

was "The Relation between Spectral Characteristics and Motions of Stars," and papers were presented by Dr. Walter Baade, Dr. A. N. Vyssotsky, Dr. G. P. Kuiper, Dr. P. C. Keenan and Dr. W. W. Morgan, Dr. Cecilia Payne-Gaposchkin, Dr. R. Minkowski, and Dr. R. J. Trumpler. The discussion was initiated by Dr. Frank Edmondson and a number of others followed him. Between the two sessions the group was transported to Delaware, the city nearby, where a very satisfactory luncheon was served at Bun's restaurant. The return journey to Columbus was made in ample time for the Society dinner.

The Society dinner, the climax of the social activities, was held at the Faculty Club. The beautifully appointed parlors and lounges were at the disposal of the astronomers before the dinner. The large dining room was entirely adequate in meeting the needs of approximately one hundred forty persons very comfortably and expeditiously. Dr. Joel Stebbins acted as toastmaster and presided throughout the evening. While still in the dining room, he called upon Dr. Otto Struve, who outlined the plans, as far as they have been made, for the meeting of the International Astronomical Union which is to be held in Zurich, Switzerland, from August 11 to August 18 next summer; upon Dr. Bengt Strömgren, director of the Copenhagen Observatory, who spoke of the status of astronomical activity in Europe since World War I; upon Dr. R. J. Trumpler, who, among other things, expressed the desirability of impressing perturbations on the orbit of the Society so as to result in holding meetings in California more frequently.

The group then moved into the large common room of the Faculty Club where a screen and projectors had been set up and temporary seating provided for the large company. First, Dr. Stebbins called upon Dr. Donald H. Menzel. In the absence of a title for his remarks, Dr. Stebbins assigned one, namely, "How I won the War!" Dr. Menzel then recounted a few of his interesting experiences, which could now be told, while serving as consultant to the Army and Navy during the war in the field of radio-wave propagation. He then showed a moving-picture film made at the Harvard Station at Climax, Colorado, showing in a very spectacular and graphic way the changes which occur on the surface of the sun. The film was entitled "Explosions on the Sun." The pictures supplemented by Dr. Menzel's remarks afforded a most convincing demonstration of tremendous forces operating on the sun. This film was followed by another film and some slides, shown and described by Dr. R. Minkowski, depicting the circumstances of the loading and transporting of the 200-inch mirror from Pasadena to Palomar Mountain on November 18 and 19, 1947. In looking at the pictures one was impressed with the extremely carefully made preparations at every step, and, indeed, with the necessity for the precautions taken, because of the magnitude of the operation. Although obviously

not intended as such, the showing of these pictures became potent propaganda for the meeting to be held in Pasadena next June.

The exercises of the evening were concluded with an expression of appreciation and thanks by Dr. Cecilia Payne-Gaposchkin in very well-chosen words to the representatives of the Ohio State University and the Perkins Observatory, and to all others whose efforts were directed toward making the meeting a success and the visit a pleasant one. The hearty applause which followed the remarks by Mrs. Gaposchkin attested the unanimity and sincerity of feeling on the part of all present.

One session for papers remained on the schedule for Wednesday morning. The size of the group had dwindled somewhat by this time, but the session was continued until all the papers listed had been given. During this session Dr. Harlow Shapley appeared for the first time, having been detained by the A.A.A.S. meeting in Chicago. Dr. Struve invited him to speak. He did so, speaking principally of the progress being made in connection with legislation pending in the Congress affecting scientific activities, and, on a wider scale, of the scientific aspects of UNESCO, with special reference to a possible international astronomical observatory, very probably to be located in the southern hemisphere.

Dr. Struve then declared the seventy-eighth meeting closed.

We add a few statistics as follows: The register for this meeting contains 144 names of members and guests present. Forty-four individual papers and seven symposium papers were presented. Thirty persons, the largest number elected at any meeting thus far, were elected to membership in the Society.

The names of those elected follow.

Miss Catherine J. Barber, Harvard College Observatory, Cambridge 38, Mass.

Mr. Earle B. Brown, 14 North Ninth Avenue, Mount Vernon, New York.

Dr. Charles Russell Burrows, 116 Mitchell St., Ithaca, New York.

Mr. William Buscombe, University of Saskatchewan, Saskatoon, Sask.

Mr. Arthur D. Code, Yerkes Observatory, Williams Bay, Wisconsin.

Mr. Ralph S. Damon, 100 East 42nd St., New York 17, N. Y.

Mr. Richard H. Emmons, 925 Shorb Ave. N. W., Canton 3, Ohio.

Mr. Sol H. Genatt, U. S. Naval Observatory, Washington 25, D. C.

Mr. Arthur A. Hoag, Harvard College Observatory, Cambridge 38, Mass.

Mr. Stuart J. Inglis, Lick Observatory, Mt. Hamilton, California.

Miss Edith M. Janssen, Leander McCormick Observatory, Charlottesville, Virginia.

Mr. David Raymond Layzer, Harvard College Observatory, Cambridge 38, Mass.

Mr. E. P. Martz, Jr., Ord. Aero., Science, R.D. & T., U. S. Naval Ordnance Test Station, Inyokern, California (China Lake).

Miss Betty Ann Mateer, Leander McCormick Observatory, Charlottesville, Va.

Professor H. S. Mendenhall, Oklahoma Agricultural and Mechanical College, Stillwater, Okla.

Mr. Willard F. Mullen, Pennsylvania State College, State College, Pa.

Mr. Ali M. Naqvi, Harvard College Observatory, Cambridge 38, Massachusetts.

Dr. Guy C. Omer, Jr., California Institute of Technology, Pasadena 4, Calif.

Dr. A. Keith Pierce, University Observatory, Ann Arbor, Michigan.

Dr. Stefan Piotrowski, Harvard College Observatory, Cambridge 38, Mass.

Mr. Grote Reber, Box 4868, Cleveland Park Station, Washington, D. C.

Miss Nancy G. Roman, Yerkes Observatory, Williams Bay, Wisconsin.

Mr. Winfield W. Salisbury, Collins Radio Co., Cedar Rapids, Iowa.

Professor Charles L. Seeger, Franklin Hall, Cornell University, Ithaca, N. Y.

Dr. H. K. Sen, Harvard College Observatory, Cambridge 38, Mass.

Mr. Richard E. Strikler, 3415 38th St. N. W., Washington 16, D. C.

Mrs. Constance S. Warwick, Harvard College Observatory, Cambridge 38, Mass.

Mr. James W. Warwick, Harvard College Observatory, Cambridge 38, Mass.

Dr. Albert G. Wilson, 78 Sierra Bonita Ave., Pasadena 4, California.

Mr. Stanley P. Wyatt, Jr., Harvard College Observatory, Cambridge 38, Mass.

The Planetary Theory of Kepler

By A. PANNEKOEK

I

Kepler's planetary theory is set forth in his "Astronomia Nova" with subtitle "de Motibus Stellae Martis" (On the motions of the star of Mars). This work, made accessible to modern astronomers in 1929 through a careful German translation by Max Caspar, shows a special character different from most of the great works of science. Usually in the publication of new important researches only the results with the data and arguments are given; the discoverers keep to themselves how they arrived at them, their fruitless endeavors, their detours, their failures, and exhibit the result as a well-rounded harmonious structure, as a work sometimes of art, constructed straight forward, where all traces of the difficult searching have been effaced. Thus Copernicus, Newton, Laplace, Gauss. This is fine for study and admiration. But in this way outsiders get a wrong idea of the making of science; they do not suspect, what every scientific worker knows through his own practice, how many painful failures and long detours one must go through before finally the direct way is found which then afterwards is easily seen as the obvious truth. Kepler, differently, exposes his entire course of research, his errors, his false suppositions and their disclosure, his perplexities and new endeavors, till the simple truth springs

forward; all is laid open before the reader. Thus the book gives a true image of the growth of scientific discovery: here we see, as it were, the scientific worker in overalls.

Kepler himself in the introductory summary says of his method: "When Christopher Columbus, Magelhaens or the Portuguese, of whom the first discovered America, the second the Chinese sea, the third the way around Africa, tell us of their wanderings, we not only forgive them but would not wish to miss their stories, because then we should miss the great pleasure contained therein. So it will not be judged a fault in me that I do likewise out of the same regard for the reader. It is true that in reading of the difficulties of the Argonauts we do not experience them ourselves, whereas the difficulties and thorns in my researches will affect the reading itself. But that is the common lot of all books on mathematics, and since among men some are pleased with this, others with that, there will be some who will feel a great joy when after having vanquished the difficulties of understanding they have the entire series of my discoveries before their eyes." In reading his book we are indeed partaking in an adventurous voyage of discovery that opened a new world of astronomical progress.

Some introductory parts, the dedication to the emperor Rodolphe II, a panegyric on Tycho Brahe, a paragraph "to the reader" by Tengnagel, Tycho's son-in-law, remind us of the often difficult conditions under which the work came into being. We remember how Kepler through his first publication "Mysterium Cosmographicum"—wherein the distances of the planets from the sun were related to the five regular solids—came into first contact with Tycho; but only after Tycho had settled at Prague and Kepler by religious persecution was obliged to give up his teaching job at Graz in Steyermark, could he accept Tycho's invitation to join him and to assist in the reduction of his observations. Through Tycho's influence he was appointed "Imperial Mathematician"; and when in the next year (1601) Tycho died, he was ordered to continue Tycho's work with the instruments, under which he judged the results of observation and their reduction to be included. This brought him into conflict with Tycho's heirs who suspected that he, as a Copernican, sceptical against Tycho's world system, would not have sufficient regard for the honor of Tycho. An agreement was reached that Kepler should discuss the observations of Mars, and Tengnagel should prepare the final planetary tables—for which, however, the latter, as a high state official, lacked the time as well as the capacity. When in 1605 Kepler was ready with his work, Tengnagel after much delaying at last had to consent to the publication. Lack of money in the imperial treasury was a cause of still more delay, so that it could not appear until 1609. In his dedication, where in allegorical language Kepler tells the emperor that he brings him the war-god as a well-fettered captive, he reminds him that the campaign has to be pursued

against Mars' relatives, the other planets, so that more money is needed. The emperor had the disposal of the printed issue; since, however, Kepler had not received salary for many years and lived in great distress, he took the right himself to dispose of the printed copies, and to publish and send them to the scholars of his acquaintance.

II

Kepler starts with explaining that the apparent motion of a planet presents two irregularities, one depending on its own longitude in the ecliptic, the other, producing the alternation of direct and retrograde motion, depending on its place relative to the sun. To determine the former by eliminating the latter, oppositions are always used. But opposition to what? Ptolemy, and in his wake Copernicus and Tycho Brahe, took the opposition to the mean sun. It means that as point of reference is taken the centre of the sun's orbit with Ptolemy, of the earth's orbit with Copernicus. Kepler, however, in his first work had already emphasized that always the real sun has to be used; because the sun is not only the source of all light but also the source of power that moves the planets.

Does it make any difference? If the planet describes its orbit with constant velocity, with the sun as well as the centre of the earth's orbit different from the centre of the planet's orbit, we can make use of either of these two points in the same way. Distance of the planet's orbital centre and direction of the line of apsides differ according as to which of them is chosen as zero point of reference; but both will fit equally well. Now, however, the velocity in the planet's orbit is not constant. Ptolemy's *punctum aequans* means that in perihelion the planet moves faster, in aphelion more slowly. Now the line of apsides is a real line of symmetry in the fluctuation of the planet's velocity; so observations must be able to decide whether the line of apsides passes through the real sun or through some other point, outside the sun. The same holds for the line of nodes, the line of intersection of the plane of the orbit with the ecliptic; it must be possible to derive from the observed latitudes whether the line of nodes passes or not through the sun.

Then he speaks of the second irregularity, depending on the sun. "Thereover people have much astonished themselves, and each has advanced another explanation . . . Latin authors supposed that in the aspects and rays of the sun a force lived attracting really the other planets . . . Ptolemy made his explanation an object of numerical and geometrical treatment, but the astronishment was not removed thereby. . . . Copernicus, however, with the ancient Pythagoreans and Aristarchus, and with them myself also, we contest that the second inequality belongs to the proper motion of the planet, and we hold it to be only an appearance due to the yearly revolution of the earth about the immobile sun." And Tycho's system is strikingly characterized

thus: "Ptolemy put epicycles upon the excenters, and Brahe, on the contrary, put all excenters upon one single epicycle that is the orbit of the sun."

And, he continues, "I will in the following demonstrations apply all three forms from these authors. Tycho, when I advised him to do so, replied that he would have done so by himself, also if I had been silent. And when dying he asked me, whom he knew to adhere to the Copernican system, to accomplish all the demonstrations for his system also." This Kepler did. For us the reading is thus made more cumbersome, but for his time, when among astronomers there was much strife over the true world-system, it was certainly necessary.

III

In the second part of his book Kepler tells first how it came about that he should treat the motion of Mars. In all these apparently chance events he sees a Divine Providence, leading him to Prague, where Tycho's pupil Longomontanus was occupying himself with Mars, but could not succeed and then instead took up the moon's motion. Kepler had some misgivings about erroneous methods of reduction and began by testing carefully all corrections. From Tycho's observations he found the parallax of Mars never to exceed 3′, though up to that time the much smaller solar parallax had been assumed always to be 3′. He found by different methods that the inclination of the orbit was always the same, nearly 1° 50′, hence all assumptions of a variable inclination, which would have complicated everything, could be rejected. He derived anew all the oppositions from 1580 to 1600, now oppositions to the sun itself, and he completed the list with the oppositions of 1602 and 1604 deduced from observations by himself and his friend David Fabricius at Emden. This table of oppositions formed the basis further on of all his work. They afford the longitudes of Mars at different moments as seen from the sun. The variations in angular velocity are visible here at first sight.

TABLE OF OPPOSITIONS OF MARS

Time				Longitude	Latitude	Long. Computed	Difference	Lat. Comp.
			h m	° ′ ″	° ′	° ′ ″	′ ″	° ′
1580	Nov.	18	1 31	66 28 35	+1 40	66 28 44	−0 9	+1 45½
1582	Dec.	28	3 58	106 55 30	+4 6	106 57 4	−1 34	+4 3½
1585	Jan.	30	19 14	141 36 10	+4 32½	141 37 46	−1 36	+4 30½
1587	March	6	7 23	175 43 0	+3 41	175 43 16	−0 16	+3 37
1589	Apr.	14	6 23	214 24 0	+1 12¾	214 26 12	−2 12	+1 5½
1591	June	8	7 43	266 43 0	−4 0	266 43 51	−0 51	−3 59½
1593	Aug.	25	17 27	342 16 0	−6 2	342 16 42	−0 42	−6 3¾
1595	Oct.	31	0 39	47 31 40	+0 8	47 31 54	−0 14	+0 5½
1597	Dec.	13	15 44	92 28 0	+3 33	92 28 3	−0 3	+3 20
1600	Jan.	18	14 2	128 38 0	+4 30¾	128 38 18	−0 18	+4 30½
1602	Feb.	20	14 13	162 27 0	+4 10	162 25 13	+1 47	+4 7¾
1604	March	28	16 23	198 37 10	+2 26	198 36 43	+0 27	+2 18¾

Ptolemy had represented this variation by a *punctum aequans* situ-

ated at a distance $2e$ from the sun (in his case the earth), with the centre of the circle exactly midway between them. Though Kepler in his "Mysterium Cosmographicum" had given reasons why this bisection should be exactly correct, he wished to test it by means of Tycho's highly accurate places. Ptolemy had needed three oppositions; since Kepler had to determine one unknown more, the ratio of division of the distance sun-equant by the circle's centre, he had to use four data. For four chosen moments (the oppositions of 1587, 1591, 1593, 1595) he knew the directions as seen from the sun, as well as the directions as seen from the equant since the latter increase proportionally to the elapsed time. The problem to find from these data the direction of the line of apsides and the two distances, from the circle's centre to the sun and to the equant, cannot be solved by a direct method; Kepler had to solve it by trying various suppositions, in successive approximations. "If this cumbersome mode of working displeases you" he says to the reader "you may rightly pity me, who had to apply it at least 70 times with great loss of time; so you will not wonder that the fifth year is passing already since I began with Mars. . . Acute geometers equal to Vieta may show that my method is not at the level of art. . . May they solve the problem geometrically. For me it suffices that . . . to find the way out of this labyrinth, instead of the torch of geometry I had an artless thread guiding me to the exit." The result of these computations was that the total eccentricity amounts to 0.18564, that the sun is 0.11332 and the equant 0.07232 distant from the centre, whereas the longitude of aphelion (for 1587) is 148° 48′ 55″. How exactly these elements represent the data, and so may be used to compute exact longitudes as seen from the sun, may be seen from the Table, where the remaining differences between observation and computation are given in the 5th column. "So I state, that the places of opposition are rendered by this computation with the same exactness as Tycho's sextant observations are exact which, through the considerable diameter of Mars and the insufficiently known refraction and parallax, are affected by some uncertainty, surely as much as 2′."

Thus Chapter 18 closes. And then Chapter 19 begins with the words: "Whoever could think it to be possible? This hypothesis so well in accordance with the oppositions, yet is wrong." Ptolemy in bisecting the eccentricity was right. This appears at once when the latitudes at opposition are used, which though not quite are yet sufficiently accurate for the purpose. The ratio of the computed inclination as seen from the sun and the observed value seen from the earth, is the ratio of the distances; making this computation for aphelion (1585) and perihelion (1593) Kepler finds the eccentricity of the sun between 0.080 and 0.0994, at variance with the value found from the oppositions. Computing, on the other hand, the oppositions in the case of a bisection of the total eccentricity, he finds for 1582 a longitude

of 107° 4¾′; deviating nearly 8′ from the previous computation and 9′ from observation.

"From this so small deviation of 8′ the cause is apparent why Ptolemy could be content with bisecting the eccentricity. . . Ptolemy did not claim to reach down beyond a limit of accuracy of ⅙° or 10′ . . . It behooves us, to whom has been given by divine benevolence such a very careful observer in Tycho Brahe, in whose observations an error of 8′ of Ptolemy's computation could be disclosed, to recognize this boon of God with thankful mind and use it by exerting ourselves in working out the true form of celestial motions. . . Thus these single 8 minutes indicate to us the road towards renovation of the entire astronomy; they afforded the materials for a large part of this work."

The demonstration is made more rigid by taking other observations at such times as Mars was in aphelion and perihelion, in addition to the oppositions. Here for the first time the trigonometric determination of distance is made use of, which will remain the backbone of his work. In the triangle Sun-Mars-Earth the direction of each side is known: Earth-Sun through observation of the sun (laid down in Tycho tables), Earth-Mars through observation of Mars, and Sun-Mars from his elements. Then from the angles known the ratio of the sides can be computed; taking the distance Earth-Sun from Tycho's tables, which though not entirely good are sufficient for the purpose, he finds the distance Sun-Mars. From these distances again the eccentricity of the sun's position is found to be comprised between 0.08377 and 0.10106, about half the total eccentricity. So he stands before the enigma that computation by the elements, which represent the oppositions perfectly, cannot be true—further on he calls them the vicarious hypothesis — whereas the true values do not represent the oppositions. And he concludes by saying: "Thus what we first had built out of Tycho's observations we had to demolish afterwards because of other observations of the same observer; this by necessity happened to us because on the authority of earlier masters we followed some probable but in reality wrong course. So great pains had to be taken by imitating the earlier masters."

Kepler then gives considerations how a wrong hypothesis can give good results. What really is the matter, we can easily see when using modern analysis that develops the irregularities in longitude in a series of increasing powers of the eccentricity. Since $e = 0.10$ corresponds to 5¾°, $e^2 = 0.01$ to 34′, $e^3 = 0.001$ to 3′.4 of arc, we have only to consider the first and second power; then in well-known notation $v - M = 2e \sin E + \frac{1}{4} e^2 \sin 2E$. In Kepler's hypothesis of a circular orbit with the centre at distances e_1 and e_2 from equant and sun we have $v - M = (e_1 + e_2) \sin E + \frac{1}{2} (e_2^2 - e_1^2) \sin 2E$. Taking $e_1 + e_2 = 2e$ we will have the right result at $E = 90°$ whatever the separate values. To make both expressions identical we have to take $e_2^2 - e_1^2 = \frac{1}{2} e^2$,

hence $e_2 = 9e/8$ and $e_1 = 7e/8$, *i.e.*, for $2e = 0.1856$, $e_2 = 0.1044$, $e_1 = 0.0812$, somewhat less different than Kepler's values. For $e_1 = e_2$ the second term disappears, and in the octants, at $E = 45°$, the error is $\frac{1}{4}e^2$, corresponding to 7.5′.

IV

Since uncertainties in the orbit of the earth spoiled the exactness of his computations on Mars, Kepler now turns to a closer examination of the former. It was necessary, besides, because with Ptolemy and Tycho the earth had no equant, whereas as a planet it should not be different from the other ones. He applies the trigonometric method to observations where Mars occupied the same place, the earth different places, in their orbits; thus he found an eccentricity 0.01837 (the five decimals are not an indication of precision but a consequence of Kepler taking always the radius 100,000). Since the fluctuations in angular velocity afforded to Tycho an eccentricity 0.03584, twice as large, it appears that the earth too has an equant. This enables him to construct tables giving the exact distances and longitudes of the sun. But at the same time it leads him to physical and philosophical considerations.

At aphelion, at large distance from the sun, a planet moves more slowly, at perihelion it moves more rapidly, exactly in inverse proportion to the distance. Distance is primary, cannot be dependent on velocity, but velocity must be an effect of distance. The force determining the velocity cannot have its seat in the planet, but proceeds from the central body, the sun. After the lever-principle a planet at greater distance is moved by the solar force with greater difficulty, hence needs more time to describe a certain arc. This physical explanation shows, Kepler says, how he was right to relate all motions to the bodily sun instead of to a void point; and at the same time how Tycho was wrong in having the heavy sun describe an orbit about the earth. The sun is not only the source of light and heat for the entire planetary system but the source of force also. Light and force, both immaterial, expand in space. Since light expands over spherical surfaces, going upward and downward, to all sides, it decreases with the second power of distance. Solar force, on the contrary, expands along circles in the ecliptic, not upward and downward, driving the planets in longitude only through the zodiac, hence decreases with the distance itself. We can understand this solar force, if we assume that the sun rotates about an axis and thus draws along the planets in the same direction, more slowly as they are farther distant. If one asks after the nature of this force he has to give attention to the magnetic force. It is a directing force, as if the magnet consists of threads or fibres; the sun too does not attract the planets—else they would fall into the sun—but directs their course through a sideway force as if it consists of annular magnetic fibres. This is more than an analogy, since Gilbert has found that

the earth, which in the same way directs the moon in its orbit, is a magnet.

It would not be right for a modern astronomer to consider these discourses as vain phantasmas. Kepler was not an arid computer, set on simply representing the celestial motions in rows of exact numbers. What mattered to him was physical understanding of things. Not incidentally he put as the full title of his book "Astronomia nova aitiologetos seu physica coelestis" (New astronomy causally explained or celestial physics). His speculations are of the same kind as later on in the 17th century often came up, product of new inquisitive impulses, far superior to the sterile scholasticism of the preceding century that still occupied the academic chairs. On account of his speaking on a force proceeding from the sun he sometimes, wrongly as we see, has been called a precursor of Newton; he was rather a precursor of the natural philosophy of the 17th century. What appears in Descartes' vortex theory as a broad and vague philosophical speculation, in Kepler's ideas has the freshness of direct precise conclusions imposed by the facts of experience.

Whilst the sun produces the general circular motion, it is the planet itself that is responsible for its special irregularity, the alternation of smaller and greater distance. In following up these trends of thought Kepler's ideas often become vague and contradictory; thus he speaks of the spirit or essence of the planet that has to attend to the apparent size of the sun. More important it is that he develops here a new method of computation. The equant as a temporary implement has to be discarded and replaced by the dependence of the velocity on the distance from the real sun. The time spent on a small arc of the orbit is proportional to the distance; so, to have the total time for a longer arc, all the intervening distances have to be summarized. This is a problem of integration—"if we do not take the sum total of all of them, of which the number is infinite, we cannot indicate the time for each of them"—which he first solves by numerical summation. But then he substitutes for the sum total the area between the limiting radii, though this is not exactly the same thing; the area can easily be computed as a circular sector diminished by a triangle. This affords the "physical part" of the inequality; the "optical part" results from that the arc described is viewed from the sun instead of from the centre.

V

Kepler now returns in the 4th Book to Mars, and computes more exactly the distance in aphelion and in perihelion each from five observations, when the earth stood in different positions. He finds them 1.66780 and 1.38500 radii of the earth's orbit, with longitude of aphelion 148° 39′ 46″. Then the radius of Mars' orbit is 1.52640 and the eccentricity $0.1414 : 1.5264 = 0.09264$, almost exactly half the total eccen-

tricity 0.18564. Computing now for the octant (E = 45°)* to which belongs a "mean anomaly" proportional to the elapsed time, of 41° 14′ 48″, he finds the "adjusted anomaly" (our "true anomaly") to be 49° 0′ 35″. The vicarious hypothesis gives 48° 52′ 34″, so again, as could be expected, we have the old difference of 8′. He now recomputes the triangles formerly used for the distance of the earth, now, conversely, for the distances of Mars; at longitudes 44°, 185°, 158° he finds 1.4775, 1.6310, 1.66255. Computation by means of the circle with the above elements affords 1.48539, 1.63883, 1.66605. Observation thus shows the distances of Mars from the sun to be smaller than what follows from the circular orbit. And the differences cannot be ascribed to chance errors. "I speak to you, expert astronomers who know that evasive sophistries, so common in other sciences, in astronomy are available to none." Sideways the planet took its course within the circle described through aphelion and perihelion. "The matter is obviously this: the planetary orbit is no circle; to both sides it goes inward and then outward till in the perigee the circle is reached again. Such a figure is called an oval."

And now at once he sees the way out of the contradictions through the following explanation. An eccentric circle can be considered as a centric circle combined with an epicycle described backward in the same time, whereby the epicycle's radius keeps the same direction in space. Now the circle produced by the sun's force is described more rapidly where the distance is small; the epicycle being the action of the planet itself, keeps a constant velocity. So from perihelion onward the angles described by the epicycle's radius are smaller than those described by the radius from the sun; the result is, as shown by Figure 1, that the planet gets inside the eccentric circle. Kepler prefers another form of construction for this idea; when the planet from perihelion A (in Figure 2) through the stronger solar force has been propelled in direction as far as the radius SP_1 it has changed by itself its distance not more than corresponds to the smaller way from A to B_1 (B_1 determined by arc AB_1 and area CAB_1 = area SAP_1 proportional to the time); making $SP_1 = SB_1$ we see that its place P_1 is situated within the circle. At an anomaly E of 90°, where the distance SP_2 is equal to SB_2, the figure can show that the point P_2 is situated an amount of e^2 (0.00858 for $e = 0.09264$) inside the eccentric circle.

The computation, however, now becomes extremely troublesome; the resulting oval is somewhat egg-shaped, and to compute the areas we have first to know the unknown places. In the case of an ellipse the areas could be computed easily because elliptic sectors are proportional to circular sectors. Since the deviations of the oval from an ellipse are

*Kepler, following the old use, with Ptolemy and Copernicus counts all his anomalies from the aphelion. We have taken them, after modern usage, from perihelion; hence the values given here for the first octant 45° are the supplements of Kepler's values for 135°.

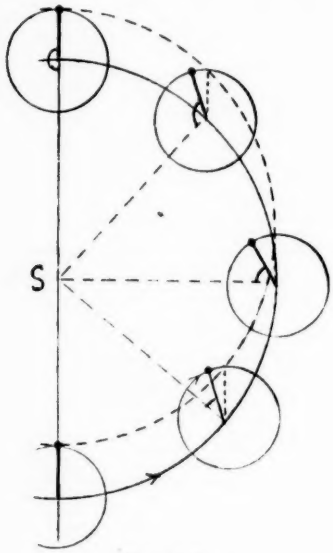

FIGURE 1

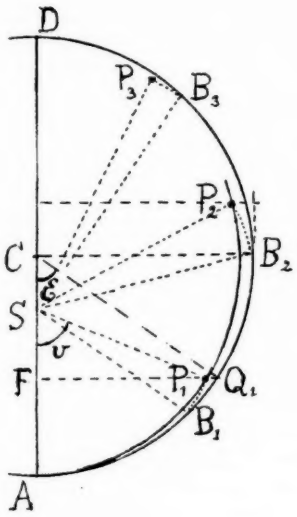

FIGURE 2

not large, Kepler decides to neglect them and make his computations as if it were an ellipse. His computation, if we express it in modern notation, comes down to the following formulas

$$\frac{\text{time AP}}{\text{per. rev.}} = \frac{\text{M (Mean an.)}}{2\pi} = \frac{\text{area ACB} = \text{ASP}}{\text{area ellipse}} =$$

$$\frac{\text{area ASQ}}{\text{area circle}} = \frac{\tfrac{1}{2}(\text{E} - e \sin \text{E})}{\pi},$$

hence $\text{M} = \text{E} - e \sin \text{E}$. Then the angle $\text{ASP} = v$ is found by the geometrical relation

$$\text{tang. } v = \text{FP/SF} = (1 - 0.00858) \text{ sin E}/(\cos \text{E} - e).$$

For $\text{E} = 90°$ the computation leaves only insignificant differences. For $\text{E} = 45°$ he finds $\text{M} = 41° 14' 48''$ and $v = 48° 45' 55''$, whereas the vicarious hypothesis gave $48° 52' 34''$, nearly $7'$ larger. Thus again it is wrong in the octants, as much as before, but now in the opposite direction. Carefully he scrutinizes whether the cause of the difference could be detected in his approximate methods and suppositions; but nothing is found. And now he tells the reader that indeed he has got on a wrong track. "As soon as Brahe's most precise observations had taught me that the orbit was no exact circle but to both sides curved more inwardly I at once believed I knew the natural cause of this

deviation. . . Because for eagerness I was blind. . . I stuck to the first cogitation offering itself that looked so wonderfully probable on account of the uniformly described epicycle. So I came into a new labyrinth from which we will have to find a way out in this and the next chapters."

Indeed his bent for understandable physical causes had played a trick on him and cost him a couple of years of needless labor. And in his war-metaphoric language he tells how he thought to have triumphed and locked the foe in the fetters of eccentric equations and the prison of numerical tables, but now saw him breaking loose from his prison and resuming his liberty. So a new campaign is necessary.

VI

First he resumes and improves his former computations: longitude of aphelion 149° 0′ 40″ (for 1600), distance in aphelion 1.66465, in perihelion 1.38234, radius 1.52350, eccentricity 0.09265. Comparing now the distances derived from observations far from the apsides with the values computed in the oval orbit, the latter appear to be too small, nearly 0.00660. The oval is situated too much inside, nearly the same amount as the circle was too much outside relative to the places derived trigonometrically. The real orbit must be situated nearly in the midst between the circle and the oval, what corresponds to the computation of the octants where circle and oval also deviate an equal amount to different sides. The maximal breadth of the deficient sickle is not 0.00858 but the half of it, 0.00429. "When I pondered ever again over the fact that my triumph over Mars had been futile, my eye fell upon the secant of 5° 18′, which is the maximal optical inequality. When I perceived it to be 1.00429 it was as if I awoke from sleep and saw a new light."

The breadth of the sickle cut off at both sides from the circle is not e^2 but $\frac{1}{2}e^2$. In an ellipse with eccentricity e half the short axis is $\sqrt{1-e^2} = 1 - \frac{1}{2}e^2$. The sun thus occupies the focus of the ellipse; Kepler does not state it expressly but it is self evident now. Again he tries to find physical explanations: that planetary magnetism makes the attraction larger when one of the poles is inclined toward the sun, and smaller for the other pole; but this does not fit in with the case of the earth. Or a planetary spirit is assumed that feels out the distance of the sun or the described road; but here the arbitrariness of behavior is too great. Kepler as a phantastic thinker always tests his phantasies critically by means of empirical data.

Now the mathematics of the ellipse are worked out. In consecutive geometrical lemmas he demonstrates that in this elliptic orbit the distances from the sun vary just as if the planet with regularly increasing E moved regularly up and down on the diameter of an epicycle—our modern equation $r = a\,(1 - e \cos E)$. Then, that the summation of all

the distances for regularly increasing E results in precisely the area of the sector swept over by the radius—we say that integration of $r = a\,(1 - e\cos E)$ produces $E - e\sin E$. Extensively he shows that all the neglected small differences in former approximations cancel out exactly in the elliptic motion. He excuses himself for the difficulty of these demonstrations. "I would not leave these things untouched though they are very difficult and not very necessary for the practice of astrology, which many consider the only goal of celestial philosophy." And he advises to read Apollonius. Then he expounds the formulas for computation, the same as given already above (with the change only of the numerator factor $1 - 0.00858$ into $\sqrt{1-e^2} = (1 - 0.00429)$; but what was a make-shift there now is the sure basis of reality.

For given eccentric anomaly E it all goes smoothly and easily; but not if we have to make the computation for a given time. The finding of E from $E - e\sin E = M$, afterwards called Kepler's equation, is recognized and put forward by himself as a difficult problem not solvable by the ordinary methods. "If the mean anomaly is given no geometrical method exists to find the adjusted and the eccentric anomaly. For the mean anomaly is composed of two areas, a sector and a triangle. . . If their sum total is given, we cannot tell what is the value of the arc and what of the sine belonging to this sum, if we have not ascertained beforehand what area belongs to a given arc, *i.e.*, if we have not constructed tables to work with them afterwards. Such is my opinion. The less handsome this method may look geometrically, the more I exhort the mathematicians to solve for me the following problem: . . . to divide in a given proportion the area of a semicircle by a line from a given point of the diameter. I am content with my conviction that a solution *a priori* is not possible on account of the dissimilarity of arc and sine. Whoever will show me my error and a way out will be to me as great as Apollonius."

In the last chapters there remains to derive the situation of the plane of the orbit. He finds for the longitude of the ascending node $46°\ 46\frac{1}{3}'$ and for the inclination $1°\ 50'\ 25''$. The latitudes observed in the different oppositions are, as is shown in the table, well represented by these elements. The remaining deviations are larger than for the longitudes, due mostly to uncertainties in parallax and refraction. Kepler points out that a parallax of Mars larger than some few minutes would spoil this concordance. Finally the observations of Ptolemy are used to determine the secular variation in the line of apsides and the line of nodes.

VII

Something must be said, besides, on the introduction printed in the front but undoubtedly written after the work was finished. Here Kep-

ler comes forward in defense of Copernicus' world system, though criticizing its details, and he refutes the arguments put forward by the theologians as well as those based on Aristotle's philosophy, as did Galileo in a different way afterwards. "A mathematical point, albeit the world-centre, cannot move or attract heavy bodies." The true doctrine of gravity rests on the axioms that every body outside the field of action of another body rests on its place; that gravity is the impulse of cognate bodies to approach one another and unite; that gravity is directed towards the centre of the earth, wherever it may be; that two stones, somewhere in the world outside the action of others, just as two magnetic bodies, would unite at an intermediate place, in such a way that the approach of each is proportional to the mass of the other. All this looks very modern. "If the earth and the moon were not kept in their orbits by some animal or other force, the earth would rise toward the moon over 1/54 (equal densities supposed), and the moon would descend towards the earth over 53/54 of their distance and there they would unite. Should the earth cease to attract its waters, all the oceanic water would be drawn up and flow into the moon." Then follows a striking explanation of the tides out of the moon's attraction, including the retardation at the coasts of the continents. And the same ideas are applied to the motion of loose bodies on earth: "thus a body thrown up vertically returns at its place . . . the earth cannot have escaped below it but draws along the objects within the air, since by a magnetic force they are no less fettered than as if in contact with her."

Also on account of these statements on gravity often expressed in so modern a way, Kepler has been considered a precursor of Newton. It is clear, however, that the principle of inertia is entirely lacking, and that gravity here is not determining orbital motion. Gravity and orbital revolution are entirely separate phenomena. The great step to combine them into one was left to Newton.

The Invention and Early Development of the Achromatic Telescope

By HENRY C. KING, B.S.C., F.R.A.S., F.B.O.A.

When William Herschel became interested in astronomy his first thoughts were to procure a good telescope. The difficulty of managing the long tubes of the simple refractors of this period led him to consider reflectors, but the prices asked by the London opticians for a reflector of moderate size was much more than he thought proper to give. There was, in fact, no optician capable of producing a reflector of large aperture and good performance; Herschel had no alternative but to make his own. This was in 1773. James Short, the only instru-

ment-maker capable of making a large reflector, had died five years previously. In his workshop just off the Strand, Short had produced reflectors to Gregory's design up to 12 inches aperture but he apparently destroyed all his tools and records before his death. His brother, Thomas Short, carried on the business but seems to have produced nothing equal in size and quality to the instruments of his predecessor. At the premises of John Bird in the Strand, Herschel might have found a few small reflectors, but Bird concentrated on precision instruments—mural quadrants, sextants and the like—in the construction of which he excelled. Burton, Cuff, Troughton, Martin, and Champneys, and the more progressive London opticians of the latter half of the 18th century would probably make a small reflector—at a price—but would, no doubt, advocate the purchase of a simple refractor, estimating its power by its length.

We are all familiar with the story of Herschel's work with the reflector—how he chose the Newtonian form for its simplicity, cast, ground, and polished his own specula and how, before the close of the century, he made reflectors of all sizes up to 48 inches aperture. The development of the reflector, in his able hands, was outstanding. Not so the refractor. This instrument, as an aid to research in physical astronomy, was completely neglected. We shall see presently how the Dollonds gave it new life when they gave it achromaticity, but the infant was almost strangled from birth for lack of optical glass wherewith to make object-glasses.

The slow development of the refractor until about 1750 was due to the blind acceptance of Newton's generalisation that refraction is always accompanied by dispersion. For nearly a hundred years the weight of his authority discouraged any attempt to prove that his deductions in this matter were wrong. There was one exception, the timid refutation made in 1695 by David Gregory, Savilian professor of astronomy at Oxford. "Perhaps it would be of service," he wrote, "to make the object-glass (of a telescope) of a different medium, as we see done in the fabric of the eye; where the crystalline humour (whose power of refracting the rays of light differs very little from that of glass) is by nature, who never does anything in vain, joined with the aqueous and vitreous humours (not differing from water as to their power of refraction) in order that the image may be painted as distinct as possible upon the bottom of the eye." Gregory was, of course, under the impression that the optical system of the eye was achromatic. We now know that this is far from the truth.

Similar reasoning led Chester Moor Hall in 1729 to design an achromatic lens consisting of a convex crown and a concave flint lens. Hall was a lawyer who dabbled, with much success, in natural science. He was especially interested in mathematics and optics and was able to compute the curves of his lenses. About 1733 he commissioned George

Bass, a London optician, to make his objectives, but Bass, like many of his fellow opticians, lacked both the initiative and practical skill to improve upon the original design. These glasses, made by Bass and a few other working opticians, had a focal length of 20 inches and a diameter of 2½ inches. Hall, modest and retiring in the extreme, left no account of his work and took no steps to get public recognition. He spent several years in the improvement of optical instruments but we know little or nothing of his other work.

John Dollond's interest in achromatism was later but quite independent from that of Hall. He was born in very humble circumstances. Following the revocation of the Edict of Nantes in 1685 his father, Jean Dollond, fled to England and joined the silk-weaving community then growing up in Spitalfields, London. As a boy John Dollond developed strong scientific leanings and in his spare time studied mathematics, optics, and astronomy. By 1750 he was well known in scientific circles in London for his wide optical knowledge. Three years previously the distinguished mathematician, Leonhard Euler, knowing nothing of Hall's work, communicated a memoir to the Berlin Academy of Science in which he endeavoured to show that it was possible to correct both spherical and chromatic aberration in object-glasses. Like Gregory, he was led to this idea by a consideration of the supposed achromatism of the eye. He evolved an object-glass consisting of two meniscus lenses with water between them and calculated that colour effects would be nullified by the combination. Dollond received a copy of this paper through his friend, James Short, and hastened to inform Euler that he was in error since Newton had expressly stated that refraction was impossible without dispersion. An interchange of letters followed during which Euler admitted that his theory was based on a law of refractivity different from Newton's but that this difference was very small. He stated that his new glasses were better than single lenses for freedom from colour but had marked spherical aberration. "Our eminent Lieberkuhn," he wrote, "is applying himself to the working of glasses of which the curvature of the surfaces decreases from the middle towards the edge, and he foresees great improvements. For these reasons, I think, my theory suffers nothing on this account." Clairaut, the French astronomer and mathematician, took great interest in the discussion and concurred with Dollond in thinking that Euler's speculations were more ingenious than useful.

In 1755 Klingenstierna, a Swedish mathematician, wrote to Dollond to say that he thought Newton had been misled in his dispersion experiments. Newton's results applied to prisms of small apical angle but were far from correct for larger prismatic angles. Klingenstierna's letter, coupled with the information which he received at this time from the optician, R. Rew, about Hall's work, completely changed Dollond's attitude. He at once put the whole matter to experimental investigation

and in 1758 read his important "Account of some Experiments concerning the different Refrangibility of Light" to the Royal Society. His experiments completely disproved Newton's conclusions. "I cemented together two plates of parallel glass at their edges, so as to form a prismatic or wedge-like vessel, when stopped at the ends or bases . . . I placed therein a glass prism with one of its edges upwards, and filled up the vacancy with clear water. . . As I found the water to refract more or less than the glass prism I diminished or increased the angle between the glass plates, till I found the two contrary refractions equal; which I discovered by viewing an object through this double prism. . . Now according to the prevailing notion, the object should have appeared in its natural colour . . . but the experiment fully proved the fallacy of this received notion, by showing the divergency of the light by the prism to be almost double of that by the water; for the object, though not all refracted, was infected with prismatic colours as if it had been seen through a glass wedge only, whose refracting angle was near 30 degrees."

To obtain refraction without dispersion, Dollond saw that he would have to increase the angle of the water prism an impracticable extent. He therefore chose smaller-angled prisms of about 5 to 10 degrees and, on arranging a combination for the non-displacement of objects seen through it, found that the dispersion due to the glass was greater than that of the water. He correctly inferred that the dispersion of glass was greater than that of water; by increasing the angle of the water prism he obtained refraction without dispersion.

The next step was to compute, grind, and polish object-glasses compounded of two spherical lenses with water between them. These glasses were "free from errors arising from the different refrangibility of light" but "the images formed at the foci were still very far from being so distinct as might have been expected from the removal of so great a disturbance." Dollond realized that much of the trouble was due to the large amount of spherical aberration due to the short radii of curvature of the spherical surfaces. He now replaced the water by a flint glass lens. In 1757 he had found that a 25-degree flint prism and a 29-degree flint prism gave little resultant deviation but strong dispersion when combined together. Further crown prisms were made and by trial and error one was found which, when combined with the other two, rendered the triple combination achromatic. From this arrangement he deduced that the focal length of the convex crown to the focal length of the concave flint must be as the ratio 6:4, that is, in the ratio of their dispersive powers. To reduce spherical aberration he "plainly saw the possibility of making the aberrations of any two glasses equal; and as in this case the refractions of the two glasses were contrary to each other, their aberrations being equal would en-

tirely vanish." On these principles Dollond made his first achromatic lenses.

In 1750 Peter Dollond, the eldest son of John Dollond, "having received some information upon philosophical subjects from his father and observing the great value which was set upon his father's knowledge in the theory of optics by professional men" opened a small optical business in Vine Street, Spitalfields. Two years later he was joined by his father and they moved to a house, "The Sign of the Golden Spectacles and Sea Quadrant," near Exeter Change in the Strand. Here John Dollond performed his valuable experiments and made his first achromatic lenses. His paper (previously quoted) was sent to James Short who passed it to the Royal Society. Although this was not Dollond's first contribution to this august body, the practical importance of its contents were so great that he was awarded the Copley Medal and elected a Fellow of the Society.

Peter Dollond was an astute business man and urged his father to apply for a patent for the new telescope. In this he was successful, the patent covering the "new invented method of refracting telescopes, by corresponding mediums of different refractive qualities, whereby the errors arising from the different refrangibility of light, as well as those which are produced by the spherical surfaces of the glasses, are perfectly corrected." At the same time, for financial reasons, Dollond and Son received a partner into the business, but the alliance was of short duration. The patent was small comfort to the optical fraternity in London. They saw the new house flourishing and envied its success. Benjamin Martin straightway advertised telescopes by "Doland" and George Adams sold telescopes "with 5 or 6 glasses" at a guinea a foot. All these early telescopes, termed "achromatic" by the amateur astronomer, J. Bevis, had small objectives owing to the scarcity of suitable discs of flint glass. Owing to their compact size and novelty they were in such demand that Peter Dollond moved to larger premises at 59, St. Paul's Churchyard.

John Dollond died on November 30, 1761, and thus did not live to see the fruits of his invention. He gave much thought to the improvement of the "divided object-glass micrometer" or "heliometer" and in 1754 read to the Royal Society his important paper on "An Instrument for measuring Small Angles." For the twin object-glass proposed by Bouger and S. Savery he substituted a single lens divided exactly in half, each section movable along its base. The advantages of this arrangement were that larger object-glasses could be made and so fitted to the end of the tubes of Short's Gregorian reflectors. Dollond proposed three types of divided object-glass micrometer. "In the first place, it may be fixed at the end of a tube of suitable length to its focal distance, as an object-glass; the other end of the tube having an eye-glass fitted as usual in astronomical telescopes. Secondly, it may be

applied to the end of a tube much shorter than its focal distance by having another convex glass within the tube, to shorten the focal distance of that which is cut in two. Lastly, it may be applied to the open end of a reflecting telescope." Only the first type has been of real service to astronomy; the Oxford and Pulkowa heliometers were designed on this principle.

What little we know about Dollond's private life and character is due to the Reverend John Kelly who in 1785 married Louisa, Peter Dollond's eldest daughter. "In his appearance he was grave, and the strong lines of his face were marked with deep thought and reflection; but in his intercourse with his family and friends he was cheerful and affectionate; and his language and sentiments are distinctly remembered as always making a strong impression on the minds of those with whom he conversed. His memory was extraordinarily retentive, and amidst the variety of his reading he would recollect and quote the most important passages of every book which he had at any time perused." Dollond was always interested in astronomy and his death occurred at a time when he was reading one of Clairaut's papers on the theory of the moon's motion "on which he had been intently engaged for several hours." "He was seized with apoplexy" Kelly concludes, "which rendered him immediately speechless and occasioned his death a few hours afterwards."

Left on his own, Peter Dollond soon found himself in trouble with the other partner. The latter's unsatisfactory conduct led to his dismissal after Dollond had paid him £200. He lost no time in exploiting the market for achromatic telescopes on his own, whereupon Dollond, finding he had paid £200 to no purpose, prosecuted him. In this action he was apparently successful, for he issued a general warning that any optician who made and sold achromatic telescopes without paying him royalty would be prosecuted. At this ultimatum thirty-five opticians and instrument-makers rose as one hostile body and in 1764 addressed a petition to the Privy Council asking it to revoke the patent. In this action they were assisted by the Worshipful Company of Spectacle makers, a body set up to guard the interests of the trade. The petitioners urged that John Dollond had copied Hall's invention and that achromatic telescopes were sold before 1758. They failed in their purpose, however, and when James Champneys of Cornhill infringed the patent on the plea that it was invalid, Dollond sued him and in paying £204 damages, Champneys was reduced to bankruptcy. In this action the Court of Common Pleas agreed that Hall was the inventor—as the workmen whom he had employed bore witness—but Lord Camden observed that "it was not the person who locked his invention in his scrutoire that ought to profit by a patent for such invention, but he who brought it forth for the benefit of the public." While awaiting judgment in this case, Peter Dollond took action against Francis Watkins

and Addison Smith, opticians of St. Martin's Lane, and was again successful. The last action, in 1768, was against Henry Pyefinch of Cornhill who made achromatic telescopes regardless of the patent.

Peter Dollond took great care to urge the claims of his father but in so doing he never once recognized the earlier work of Hall. In 1789 he published a refutation of some remarks by Lalande in his "Astronomy" and by Cassini, Director of Paris Observatory. His paper was read to the Royal Society but the Council considered it too controversial for inclusion in the "Philosophical Transactions." In 1790, a letter by an anonymous author who signed himself "Veritas" appeared in the "Gentleman's Magazine." "Veritas" was obviously in full possession of all the facts. "Mr. Hall," he wrote, "used to employ the working opticians to grind his lenses; at the same time he furnished them with the radii of the surfaces, not only to correct the different refrangibility of the rays, but also the aberration arising from the spherical figure of the lenses. Old Mr. Bass, who at that time lived in Bridewell precinct, was one of these working opticians, from whom Mr. Hall's invention seems to have been obtained." This article was well received and widely read, but Dollond preferred no reply. It is not unlikely that "Veritas" was the famous instrument-maker, Jesse Ramsden, for de Rochon, who visited London in 1790, gives an account of some misunderstanding between Ramsden and Peter Dollond. Ramsden, moreover, was brother-in-law to Dollond and was well known in scientific circles in London. de Rochon himself wrote some years later an inaccurate narrative which purported to stress the claims of Hall over those of Dollond. As late as 1802 another French version appeared in Montucla's "History of Mathematics" in which Hall is given absolute priority. This account is ascribed to Lalande. But by this time the patent had become void; Dollond was head of a very flourishing optical business and had grown weary of lawsuits.

In France it was generally held that both Hall and Dollond obtained their ideas from Euler. The Swiss mathematician was at first suspicious of Dollond's claims and attempted to explain the "goodness of Dollond's telescopes to be owing to the greenish colour of the crown-glass which did not transmit all the red rays." Alternatively, it was due to the particular eyepieces Dollond employed. But in 1764, Euler admitted "I should never have submitted to proofs which Mr. Dollond produced to support this strange phenomenon, if M. Clairaut, who must at first have been equally surprised at it, had not he most positively assured me that Dollond's experiments were but too well founded."

In the year following John Dollond's death and when little was known on the Continent about his telescopes, the Petersburg Academy made the correction of the defects of simple microscopes and telescopes the subject for a prize. The prize was taken by Klingenstierna, who

published the results of some seven years study of the problem. Clairaut translated this work for the French Academy and himself repeated Dollond's experiments. He noticed how objects seen through a supposedly achromatic lens or prism were surrounded by green and red coloured fringes, an effect which he correctly attributed to the fact that the dispersive powers of the two glasses were not exactly matched. He realized that crown and flint glass had different partial dispersions and showed that the image given by any two-lens combination must inevitably be accompanied by secondary spectrum. To reduce this effect he made careful determinations of the refractive indices and dispersive powers of several glasses and deduced theorems for combining them together so as to form better achromatic doublets. His results were given to the French Academy in three classic papers. In the first, he introduced the famous "Clairaut" objective, with its equal interior curves, the radii of which were approximately 1/5th the radius of the front crown surface. His second memoir contained suggestions for other combinations, some with the flint lens in front, others very similar to types in use today. In the third memoir he dealt with the important problem of extra-axial aberrations. He found that if he corrected a lens for chromatic and spherical aberration, rays incident on the objective to the optical axis were not refracted to the focal plane but fell outside it. By tracing rays of different obliquities, he saw that they focused on a surface of complicated form. This figure is due to astigmatism and to coma, to further separate aberrations. Clairaut managed to suppress astigmatism, the worst, but he could do nothing for coma and considered it an irremediable evil of twin-lens combinations. Fortunately, a restricted field due to uncorrected coma is no great drawback in visual work, but when an extended one is required a coma-free "aplanat" is essential.

d'Alembert, another French mathematician, made similar investigations which led to the same conclusions. He showed, moreover, how spherical aberration could be corrected for two colours instead of for just yellow, but it was left to Gauss many years later to design a lens which would give good definition for all colours.

The first achromatic objective made in France was probably the one of 3 feet focus which Passement presented to Louis XV in May, 1761. Clairaut had his glasses made by the amateurs Antheaulme, de l'Estang, and Bouriot. "M. de l'Estang," he writes, "to whom I owe my first encouragement in optics, by the success my researches have met in his able hands, has also given me the satisfaction of knowing that the objective that I thought the most perfect in theory was the best in practice." This was a crown-flint doublet corrected for astigmatism, 38.3 mm. in diameter and 73.8 cm. in focal length. Two of de l'Estang's telescopes of 30 inches focus were fitted to a half-circle invented by the duc de Chaulnes. This was probably the first applica-

tion of the achromatic telescope to a divided instrument and whilst it was claimed for the circle that its divisions had an accuracy equal to those of an 8-foot quadrant, the magnification of the telescope (X 42), was equal to that of an 8-foot quadrant telescope which it surpassed in definition and size of field. Bernoulli thought the instrument rather complicated but acceded that de l'Estang and his optical colleagues made excellent glasses. Although not professional artists they worked "with infinite care and left nothing to be desired in their glasses." "Perfect success witnesses to the patience of these gentlemen, and as they are so diligent, friends to whom they have left their glasses have already profitted by their efforts. I have heard mention of telescopes of 10 and 12 feet length made by them and have seen some myself that are quite as good as those of Dollond."

John Dollond was apparently the first to try to correct secondary spectrum by the addition of a third lens in his object-glasses, but he never made his results public. R. J. Boscovitch, a Jesuit professor at Padua University, entertained similar ideas and devised an instrument which he called the "vitrometrum" for investigating residual aberrations. This consisted of a glass prism mounted so as to work against a variable water prism (he intended making the third lens a water lens) and in use was directed to the short spectrum thrown on a screen by means of an achromatic prism. By varying the angle of the water prism, Boscovitch was able to give the secondary spectrum the least possible extension.

Peter Dollond made the first triple objectives about 1763, and the following year, offered them for sale complete with tube and stand. One of his first triplets had an aperture of 3¾ inches and a focal length of 42 inches—an aperture ratio of F/11. According to Short it gave an image "distinctly bright and free from colours" when charged with a magnification of 150. Dollond made many objectives of this size, for the demand was great and he appears to have come across a pot of very fine glass from which he cut large discs free from striae and other defects. James Ferguson, the Scottish astronomer, says that this was a "vulgar error," the proprietor of the glass-house having assured his friend that the original methods of manufacture of optical glass were still being followed. At any rate, the number of discs obtained from such a pot would not have been great, and Dollond, in common with other opticians, experienced great difficulty in obtaining what were then large discs. Had he been able to secure a steady flow of blanks, his triple objective would have been even more popular, despite the high price he charged for it. Glassmakers were lucky if they could manufacture discs larger than 4 inches in diameter. The only glassworks of any importance were at Lambeth and Ratcliffe in London, and the latter were destroyed at the beginning of the 19th century to make way for a London Dock. These factories made both crown

and flint glass, but the demand had never been great enough to warrant technical research. Nor did Dollond's monopoly of the telescope trade encourage a concerted effort by the London opticians to remedy the situation. According to Ferguson, "the principal opticians"—he may well have had the Dollonds in mind—"always complain of the bad quality of the glass, but never fail to take the whole quantity he makes at their request; and that, when they renew their orders, they always desire it may be exactly the same as the last." One consolation, if such it can be called, was that conditions abroad were even worse, despite the efforts of Colbert and Cassini to encourage research in optical glass and to establish a permanent glassworks.

Maskelyne was among the first to buy one of the new 3¾-inch telescopes and was so pleased with it that he mounted it in a small room made especially for the purpose. Another, also of 46 inches focus, was purchased by the Hon. T. Beauclerc from whom it passed to Aubert. Ramsden made all the mechanical parts, Dollond the objective, which he told Kitchener was "one of the things which is to make me immortal." Kitchener bought this instrument at the sale of Aubert's equipment in 1806, and spoke of it as "one of those perfect Instruments which are rarely produced, and only attainable, by a happy concurrence of the various circumstances which combine to form these compound Object-glasses." "Since Mr. Beauclerc's 46-inch telescope has been in my possession," he wrote later, "I have had opportunities of carefully and attentively comparing it with nine achromatics of 5 feet focus, with double object-glasses of 3¾ inches in the clear aperture, with three 7 feet of 4 inches aperture, and one 10 feet of 4 inches aperture and when the test was a star, the 46-inch has always been acknowledged to be the more perfect instrument, as it showed everything the others did; and with it, some delicately minute objects could be easily discerned, which some of its competitors were not perfect enough to define at all. The superiority was most manifest when the instruments were turned to double and coloured stars." Such was Aubert's "ample fortune" that he possessed four telescopes of this size, while his observatory contained besides thirty-three other instruments! His favourite instrument was not the Beauclerc refractor, however, but another triple achromatic of the same size which also passed to Kitchener. Darquier in Toulouse had another triple objective of 3¾ inches aperture and 42 inches focus. "This telescope," he wrote in 1777, "is entirely of brass with a small star-finder fixed on the top . . . it is of admirable clearness and definition; I have often seen four of Saturn's satellites with a high magnification, and sometimes five."

From these and other statements, too numerous to mention here, it is obvious that Dollond's triple objectives were of much higher quality than anything previously produced. The third lens not only reduced secondary spectrum but gave better correction for spherical aberration.

The latter, however, can be completely corrected by only two spherical lenses—as Fraunhofer showed later—but Dollond was not aware of this. Much was expected of his glasses, both doublets and triplets. Many astronomers wished to see Herschel's double stars for themselves and began to assess the performance of their telescopes by the ease with which they could separate these objects. This meant that the corrections had to be more rigorous and the lenses more carefully mounted and centered than hitherto. This Dollond achieved and his glasses took powers of 350, or 90 to the inch of aperture, without "breaking down." One was compared with Huygens' 123-foot refractor, to the great advantage of the smaller instrument. Maskelyne found that his telescope performed as well as a double achromatic of over twice the length. Kitchener also possessed a 5-foot double achromatic of 3.8 inches aperture which was "one of the chef d'oeuvres of the late Mr. Peter Dollond, made in 1793. . . It shows the disc of the moon as white and free from colour as any Reflector;—to its perfect achromatism I attribute in great measure its power of very distinctly showing the division of the ring of Saturn." Dollond himself said of this instrument, "it is one of the best I ever made, and such as I cannot expect to be able to equal." His largest telescope was a 5-inch double achromatic of 10 feet focus. It was the only one of its size and was erected at Greenwich Observatory for observations of the eclipses of Jupiter's satellites and occultations of faint stars by the moon.

We know little of Peter Dollond's method of working his glasses. Such information, being a trade secret, he kept to himself and to his chosen employees. He once admitted to John Bernoulli, who visited his workshop in 1769, that he did not possess his father's theoretical knowledge. Bernoulli also states that Dollond worked his glasses, even those with three lenses, on the most elementary principles, and that he depended for his success on methods of trial and error. He apparently made several lenses of flint and crown glass at a time and then used those combinations which yielded the best results. This procedure, with large discs so expensive and difficult to procure, would have hardly recompensed him for his labour or produced such excellent results, and we must accord him a large measure of practical skill. Kitchener, who knew him well, called him the "Father of Practical Optics" and stated that for practical work he had "as good and as well-educated an Eye as perhaps ever Man had."

In 1783, Peter Dollond, or rather, the Dollonds, for in 1766 he was joined by his brother John, began to provide their telescopes with brass draw-tubes. They had been the first to make brass-bound mahogany tubes, a great improvement on the old paper-covered vellum tubes. Joshua L. Martin was the first to use brass draw-tubes, which he patented, and Dollond probably bought the patent from him. The process was to first place the soldered or brazed tube on a mandril

having the same diameter as that required for the interior diameter of the tube. The tube was then drawn on the mandril through holes of smaller and smaller diameter until the right thickness was obtained. With these tubes the Dollonds offered a wide range of sturdy stands from "pillar and claw" types to folding mahogany tripods with alt-azimuth heads. Prices ranged from £2 for a 1-foot telescope to over £70 for a 5-foot telescope.

Peter Dollond left his workshop at 59 St. Paul's Churchyard during his 87th year, retiring to a stately residence on Richmond Hill, a short distance from London. By this time most of the London opticians had lost their animosity towards him and he had a large circle of scientific friends. He was for many years a member of the American Philosophical Society, and in 1779 became Master of the Worshipful Company of Spectacle Makers. His retirement after many years of business activity was of short duration for on July 2, 1820, he died, only a few days after he had moved into a new house at Kensington. The business thereupon passed to his nephew, George Huggins, who later changed his name to George Dollond.

For many years after Dollond's invention the London opticians had to rely on English flint glass. Many and varied were the efforts made to improve its quality but glassmakers clung to their old methods, content to produce small discs tolerably free from major defects. Up to 1825 the English opticians were faced with an ever-increasing excise duty on both crown and flint glass and when Fraunhofer and Guinand set to work in Bavaria the initiative passed into German hands. All the energies of Sir David Brewster and of Sir John Herschel failed to save the unhappy situation; an attempt to induce Guinand to start a glass factory in England was frustrated by the latter's death in 1824. At last, but too late, the British Government decided to finance research in glass manufacture and to reduce the tax on flint glass. Michael Faraday was commissioned to experiment with new materials and methods of manufacture, but he failed to accomplish what a simple Swiss bell-maker had done with the slenderest of resources. Robert Blair turned to fluid lenses and his telescopes were greatly admired in certain quarters in Scotland. With his son's help, he tried to make fluid lenses on a commercial basis, and even took out two patents, but nothing came of his scheme owing to the opposition of many practicing opticians. Peter Barlow, professor of mathematics in the Royal Military Academy at Woolwich, also experimented with fluid media. He placed a small fluid lens of carbon bisulphide (enclosed between two carefully ground plano-meniscus lenses), some distance behind an ordinary crown glass object-glass. In 1827 he constructed a 6-inch aperture refractor and two years later a 7.8-inch refractor on this principle, but the glasses would not take powers greater than 20 to the inch of aperture and lacked permanency. Meanwhile a few of Guin-

and's blanks filtered into England, some of which came into the hands of George Dollond and Charles Tulley, an optician at Islington. These artists were able to make achromatic telescopes up to 7¼ inches in aperture; by 1850—about a century after John Dollond's invention—George Dollond made the largest achromatic refractor in England, Lord Wrottesley's 7¾-inch polar equatorial. The largest achromatic was Fraunhofer's famous Dorpat refractor of 9¼ inches aperture—that is, if we overlook the 13- and 15-inch refractors made by Lerebours of Paris. These were of such mediocre quality that the outer zones had to be masked out if anything approaching reasonable definition was required.

Sir William Herschel died in 1820 and his work on the Newtonian reflector was continued in a much more restricted way by his son, Sir John Herschel. Gregorian and Cassegrainian forms, however, did not suffer complete eclipse and small instruments of these types were made by opticians like the Dollonds, Ramsden, Nairne, and Cary. The quality of Herschel's work was not easily reached and although W. and S. Jones and W. Harris offered reflecting telescopes "for making celestial observations in the most commodious and accurate manner" their mirrors were inferior to Herschel's in performance. The growing ascendency of the achromatic telescope, its increasing use with graduated instruments, its excellence for micrometrical work on double stars (then special objects of study), led to the almost complete extinction of small reflectors. Later, the giant lenses of Merz, Cooke, and Clark rivalled the mirrors of Rammage, Nasmyth, and de la Rue but the giants of Lassell, Rosse, and Common gave the reflector a supremacy in sheer light grasp which its rival could not and never will attain.

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Some Experiments with Color in Natural and Artificial Sunspots

By JAMES C. BARTLETT, JR.

In the year 1590 the good ship, *Richard of Arundel*, was beating down the African coast, bound for Guinea, when an accidental observation was made which went down in the log as a kind of prodigy. And this is what excited the wonder of those ancient mariners:

"On the 7, at the going downe of the sunne, we saw a great black spot in the sunne; and the 8 day, both at rising and setting, we saw the like—which spot to me seeming was about the bigness of a shilling, being in 5 degrees of latitude, and still there came a great billow out of the souther board."

Thus even before the "discovery" of sunspots by Fabricius and Galileo, nature occasionally threw out broad hints that there were aspects of the sun as yet undreamed of in the dogmatical philosophy of the day; and, even after the appearance of the "Optik Glass" of Signor Galileo had settled once and for all the fact of spots in the sun, other problems arose which gave birth to controversies some of which are still unresolved. One of these legacies from the past is the question of color in sunspots. Do sunspots occasionally show color, or do they not?

It is perhaps not surprising that reports of color should have come from the earliest observers, armed imperfectly as they were with instruments of inferior achromatism or no achromatism whatever; and for this reason there seems to be a tendency to discount all reports of color in sunspots, including reports from modern times. Yet the evidence, historical and otherwise, is impressive; and, since color is one of the most nearly omnipresent facets of nature, one may wonder why it should be doubted in the case of a star which itself is plainly yellow and which, were it not for its sodium-laden atmosphere, would be as plainly blue.

Be that as it may it is a fact that quite early in the art, solar observers reported color in sunspots; and it is interesting to note that all were generally agreed on certain salient facts: color was a rarity; it was seldom vivid; it was confined largely to very large or very active spots, *i.e.*, to spots which might reasonably be looked to for something unusual. Now the same relations are found to hold good at the present time, which is at least a point in favor of the ancient observers.

As early as 1759, Messier, the unwitting founder of nebular astronomy, reported a deep brown color in "the great spot" which appeared that year and which was remarkable for a peculiar S-shaped umbra. The same color was observed from time to time during transits of

Mercury when the black body of the planet could be compared directly with spots on the sun; but the colors seen were not always the same.

In 1826, Capocci reported violet in the form of a "haze" surrounding a brilliant bridge over "a double umbra," and the present writer saw a reddish haze of much the same nature in much the same kind of spot a few months ago. In 1858, Father Secchi saw a rose-colored "promontory" in a naked-eye spot, and the work of both Schwabe and Schmidt contains many references to reddish, brownish, violet, and yellowish tints. Lockyer saw both violet and copper-red.

Young of Princeton, who published one of the best texts on the sun in the last century, was disposed to doubt the existence of color, particularly violet which he ascribed to the secondary spectrum of achromatics. As for the undoubted fact that comparison with Mercury in transit often showed spots to possess some indefinable hue, he cited observations of the planet itself which showed that it was not a black body, as theory required, but also showed a violet tinge when projected on the photosphere—though not always. He justly remarked that such a color could arise from nothing but illusion or from the secondary spectrum and was inclined to refer all spot colors to similar causes. This seems to be the prevailing attitude of modern doubters.

Yet it is difficult to see why careful and experienced observers should be occasional victims of an illusion which manifests itself in different colors at different times, particularly when other spots contiguous to the colored spot are plainly black. Moreover, it is difficult to see why color should *not* occasionally appear in sunspots.

Let us consider the matter of color *per se*. It is in itself not a cause but an effect; the effect of trains of light waves of definite and measureable wave lengths. When we say that a color is red we really mean that our eyes are perceiving a wave length of low frequency. When we say that it is violet we refer to a wave length of high frequency, and so on in all gradations for the colors in between. When we say that an object is colored, we mean that it reflects, refracts, or transmits light in some particular wave length or mixture of wave lengths. If the object is incandescent we mean that it emits light in some particular wave length and so on.

To produce color therefore it is only necessary to have matter capable of sorting out wave lengths, whether by reflection, absorption, refraction, interference, transmission, or by combinations of such methods. In the case of the sun we have a body which is *emitting* light. Taken as a whole the sun emits in all wave lengths; but different levels of the sun also emit in different wave lengths and so we have the characteristic rosy red of the chromosphere, the yellowish tone of the photosphere, and so on. Why then should it be thought remarkable that sunspots occasionally display color?

One thing which is commonly overlooked in denying color to sun-

spots is the fact that the spot is *not* black but only appears so by contrast with the photosphere. It is not even dark but, on the contrary, is extremely brilliant; the darkness and the apparent blackness alike are merely relative, as may be demonstrated by isolating a single spot from the photosphere.

This being so it follows that the spot is actually *emitting* light while also, of course, absorbing light from beneath—but it is emitting at a temperature substantially below that of the photosphere, and so it appears dark by contrast. The temperature of the umbra is low; but we should remember that it is only *relatively* low. The researches of Pettit, Nicholson, and Richardson agree in assigning absolute temperature values of from 4,800° to 4,860° to umbrae. It is clear that at such temperatures the spots must be emitting light intensely.

Color emission is definitely a function of temperature, so that on this ground alone we should expect to see color in sunspots; and conversely the fact that color is occasionally seen is what we might expect. The hue of the emitted color will depend upon the temperature, since this will determine the wave length of the emitted light. Thus it is hardly accidental that violet is associated with spots of remarkable size or intense activity, while brownish or reddish tones seem to belong rather to smaller or declining spots. In this connection, Dr. Menzel contributes the thought that spot colors may also be due to superimposition of chromospheric eruptions. This seems particularly applicable to red and violet tints.

However the color is produced, one thing is certain. In the case of emitted color it is clear that only spots of remarkably intense activity could be expected to show it—hence the fact that sunspots commonly appear black. The majority of them certainly so appear; but there are others which just as certainly do not. The writer has made a small study of such spots and published a report of color changes in the large group of February, 1946 (*Science;* vol. 103, 2684). These observations, previously reported, led to a considerable discussion at the May, 1946, meeting of the A.A.V.S.O., held at Smith College.

At this meeting the inevitable question of the objectivity of sunspot colors was raised, the general trend of opinion seeming to favor illusion as an explanation. The writer therefore undertook certain experiments designed to show whether spurious colors tend to appear spontaneously in sunspots, and if so under what conditions. The results of this inquiry, while hardly conclusive, are nevertheless interesting and may possibly have some significance.

In discussing sunspot colors it must be remembered that only a few hues are seen; violet, which predominates, various shades of brown, and occasionally reddish tones. Nothing like green, orange, light blue, or the like, is ever seen. Moreover the tints are always subdued. Hence

in approaching the matter from the experimental side only a few colors had to be considered.

The experimental work was done by steps, the first involving the sun itself as test object with the writer observing alone. For a selected period of time, all sunspots were carefully observed for color *with the helioscope screen at normal density;* but during this period all umbrae appeared black. Next, experiments were made with the helioscope to determine the effect of changing the density of the screen.

It was found that a moderate lightening of the screen had no effect on the umbrae, all of which continued to appear black, although the photosphere changed from orange to intense yellow-orange. However, when the screen was lightened *beyond the safety point, i.e.,* to a degree at which the eye was dazzled, a faint purplish, or violet tint became apparent in *all* the umbrae. Obviously this was a complementary hue, for, when the screen was restored to normal density and the eye allowed to recover, the same umbrae all appeared normally black again. It was found that this false violet could be reproduced at will by changes in the density of the helioscope screen.

Several points are worthy of note. In the first place the experimentally induced false color affected *all* the spots equally; but the writer has seen violet spots immediately adjacent to spots which were just as plainly black. In the second place the sun is not normally observed with a screen which is light enough to produce discomfort; but at no time did the experimental spots show color until the screen was lightened to this extent. Yet violet umbrae have been observed with the screen at normal density. Obviously in such cases the color was *not* due to contrast or to overstimulation of the eye.

Also the experimentally induced color was always violet; but not infrequently red and brown shades are seen with normal screen density. Moreover, the writer has observed changes from violet to red-violet, brown-violet, and back to violet in certain spots. In brief, the experimental color induced in sunspots in no way conformed in behavior and appearance to color seen under normal observing conditions.

A second series of experiments was then undertaken with a group of persons carefully selected for average color sensitivity, with a view to determining the degree to which such persons might see false color in truly black spots under conditions closely simulating normal, direct view, solar observations.

A series of colored discs on white grounds was used to determine visual color norms, and idealized sunspots were used to test for false color. In all cases standard Winsor and Newton professional artists' water colors were used to prepare the colored discs and spots. Five subjects, who kindly consented to cooperate in the experiments, had the following specifications:

Subject	Vision	Age	Occupation	Sex
A	astigmatic-myopic	48	physiotherapist	male
B	partial astigmatism	37	editor	male
C	normal	23	chemist	male
D	slightly presbyopic	33	artist	female
E	normal	25	author	male

With the exception of A, none of the subjects had any astronomical knowledge, and A's knowledge was confined to popular reading. In only one case, subject D, was there any knowledge of color. In no case did any of the subjects know the purpose of the experiments but were encouraged to believe that they were being tested for color blindness. Nor did any of the subjects realize what the idealized sunspots represented. These, by the way, were perfectly round umbrae surrounded by perfectly round, evenly striated penumbrae.

Tests were begun by having the subjects individually examine the colored test discs under normal artificial lighting, using a 75-watt reflector-type floor lamp, in order to test their color sensitivity. The colored discs were violet, blue, yellow, vermillion, Hooker's green, red-brown, and black. It was found that all had considerable difficulty with violet, three subjects calling it blue and two, purple. On the other hand, all gave correct estimates of blue and yellow. Vermillion was thought to be some shade of light orange or orange-red. In the case of green all but subject D gave correct estimates, and she called it blue-green. Red-brown was troublesome, some thinking it dark red and others merely some shade of brown. All estimated black correctly.

Next an identical series of color discs were examined as before, but through red and blue filters alternately. With the red filter, all of the subjects thought violet was black and the same for blue. With yellow, three estimates of white were given and two of pale yellow. Estimates of vermillion varied from orange and red-orange to apple red. Green they thought was black. Red-brown appeared red to all subjects. With black three correct estimates were made, but to one subject it appeared purple and to another, gray.

With the blue filter, four of the subjects still thought violet was black while one of them called it blue. None gave the right color. With blue all agreed on the color, though some saw the shade darker than others. Yellow yielded three correct estimates, one of yellow-green, and one of pale green. With vermillion, four of the subjects thought it black and one called it brown-olive. Green generally appeared bluish-green. With red-brown, four estimates of black were given and one of green. Black appeared black to three of the subjects, blue-black to one, and brown to another.

It will be observed that all of the subjects had trouble with violet, being unable to distinguish between violet (compounded of purple and blue) and straight purple or straight blue, when observing the color without filters. Three out of five thought it blue. Otherwise color

perception was very accurate. The vermillion employed was Winsor and Newton Middle Tint, which has an orange cast.

With the use of filters it is significant that all five subjects saw violet as black through the red filter, and four out of five through the blue filter. This suggests that an observer using a red or blue tinted helioscope filter would be apt to see a truly violet spot as *black*, not *vice versa*. On the other hand, when viewing black through the filters, in only one case (subject C) was violet approached as a false color; and in three cases out of five black was seen as black through either filter. On the whole a blue filter tended to give black more often with all colors save yellow and blue. Now blue is normally a color employed in tinted helioscope filters.

In order to test the appearance of colored spots as they would be seen through a neutral filter, all of the subjects were examined *singly* and *alone* by means of a series of test charts. These consisted of circular fields alternating between cadmium yellow light and cadmium yellow deep tint, representing the photosphere as seen through a neutral screen of two densities, on which appeared idealized sunspots in various colors.

Most of the fields contained a single idealized sunspot, but several represented the entire disc of the sun with several groups of spots and one represented a bi-polar spot with a black and a violet umbra immediately adjacent. The uniform diameter of all fields, including those representing the sun itself, was 6 centimeters. In the case of fields containing single spots, the diameters of the spots were alternately 1/3 and 1/6 the diameter of the field. These were employed to determine whether difference in circular area of spots relative to fixed value for the field of view would have any significant effect upon color estimates.

Each subject in turn was taken into a darkened room and given the test charts, 1 to 20 inclusive, and required to estimate the color of the spots. In all cases subjects were instructed to disregard the background color, *i.e.*, the color of the field. The charts were observed by means of a brilliant spotlight and through a monocular, achromatic eyepiece which reduced the size of the field from 6 centimeters to an apparent diameter of 2 centimeters when in sharp focus. This corresponded to a reduction of 89 percent in the circular area of the field and its included spot.

As thus observed the test charts represented a telescopic field of view, and the spotlight was purposely made slightly dazzling in order to simulate the visual effect of a telescopic field of the sun as seen through a too-light screen. In other words the tests were actually weighted in favor of the hypothesis that color in sunspots is an effect of contrast under brilliant illumination. None of the subjects knew what the charts represented.

Charts numbered 1 to 6 each represent black spots, three sets on

deep yellow and three sets on pale yellow grounds. Diameters of the spots varied uniformly, 0.1 to 0.5 centimeter, 1 centimeter, 2 centimeters. Diameters of the fields were uniformly 6 centimeters. Charts 7 to 10 represented single violet spots on deep yellow grounds; 11 to 14 represented single violet spots on pale yellow grounds, all varying in diameter as before. Number 15 was a yellow-brown spot, 2 centimeters diameter on a deep yellow ground; number 16 was a pair, violet and black, diameter 1 centimeter on pale yellow ground; number 17 was a red-brown spot, 2 centimeters diameter, on a deep yellow ground; number 18 was a black spot, 1 centimeter diameter, on a white ground; number 19 was a red-brown spot, 1 centimeter diameter on a white ground; and number 20 was a violet spot, 1 centimeter diameter, on a white ground.

Charts 1, 4, 8, and 12 represented whole discs of the sun with spot groups. Number 16 was a bipolar spot, violet and black. Numbers 18, 19, and 20 had white fields in order to eliminate the influence of background color. The subjects estimated the spot colors as follows:

Chart numbered 1 yielded two estimates of black, two of blue, and one of purple. These were for spot groups. Chart number 2, a single spot, 1 centimeter in diameter, gave four estimates of black and one of purple. Charts 3, 4, 5, and 6 gave uniform estimates of black, which was correct.

Chart numbered 8, another whole disc of the sun with small spot groups (the diameter of the field, or sun, being only 6 centimeters) gave two estimates of black, one of blue-black, and one of purple. The true color was violet. Charts numbered 9 to 14, inclusive, gave only three correct estimates of violet, thirteen of blue, one of bluish-purple, one of purple, and five of black. These charts represented single violet spots.

Chart 15, the yellow-brown, 2 centimeter spot, gave gray-green, khaki, olive-drab, tan, and green! Chart numbered 16, the bipolar violet-and-black spot, gave black-black, black-brown, black-purple, black-violet (the only correct estimates) and purple-purple. Number 17, a 2 centimeter red-brown spot, gave brownish, black, brownish-black, black, and black.

Chart numbered 18, a 1 centimeter black spot on a white ground, was correctly estimated by all the subjects. Number 19, 1 centimeter red-brown spot on white, gave red-brown, brown, brown, black, and dull red. Chart 20, a 1 centimeter violet spot on white ground, gave two estimates of blue, one each of dark blue and navy blue, and one of purple.

It will be observed that with the exception of chart numbered 1, a severe test, all truly black spots were seen as black excepting a single error by subject E; and these correct estimates of black were made on spots of varying size in fields alternately of light and deep cadmium

yellow. Neither field color nor ratio of spot to field area appeared to have any influence here. On the other hand, as foreshadowed by the preliminary color sensitivity tests, all subjects had trouble with violet and it is noteworthy that six estimates of black were made.

The influence of field color was perhaps most marked in chart 15. In this chart the yellow-brown spot on deep cadmium yellow background proved very troublesome. Subjects A and E even saw the spot as greenish or green. No subject was able to give a correct estimate, though B and D came nearest with khaki and tan. Chart 16 also gave some trouble, though D gave a correct estimate and C was approximately correct (black-purple). It is remarkable that both C and D placed the color correctly with respect to the proper spot. B saw the violet spot as brown. On the other hand, all but E saw the black spot as black. Of two adjacent umbrae, one violet and the other black, it would appear that the violet spot would not ordinarily induce false color in the black companion; and this is in good agreement with observation.

These simple tests cannot be regarded as being anything but suggestive. Still it is noteworthy that black spots when viewed through red and blue filters continued to appear black in six cases out of ten. Again the inference is that a black sunspot will very likely appear black under normal conditions of observation.

On the other hand where color was actually present in the test spots it was detected in forty-three cases out of fifty-five, though the subjects were not always able to agree upon the hue. In twelve cases out of fifty-five, colored spots were thought to be black; but in only four cases out of thirty were black spots thought to be colored; and in three of those four cases estimates were made of minute spots varying from 0.1 to 0.5 centimeter diameter on a 6 centimeter field!

Statistics, of course, may be made to prove anything, and the writer does not suggest that the above findings have any absolute significance. But the results nevertheless strongly suggest that color in sunspots should not be dismissed offhand as illusion.

In this connection, Mr. Neil J. Heines, Chairman of the Solar Division of the A.A.V.S.O., recently appointed the writer as director of an investigation into sunspot colors. Several experienced A.A.V.S.O. observers are now cooperating in this program, and we have also had the rare good fortune to secure the services of Dr. Walter L. Moore, of the University of Louisville, who is working with a 12.5-inch Clark reflector.

Last year (1946) which saw some remarkable outbursts of spot activity was also an exceptionally good year for colored spots. Still, as a measure of their rarity, of the 2,395 spots observed by the writer at Baltimore only 63 were found to be colored or only 1 in every 380. Moreover, the majority of them were accounted for in several very

active groups. It is also perhaps significant that of the 63 colored spots observed, no less than 56 were in the northern hemisphere which was also the hemisphere of the greater activity. In other words, manifestations of color appeared most frequently in the most active groups.

Some of these color observations were confirmed, at least as to the presence of color, by other observers on this program. Mr. Topham of Toronto, for instance, confirmed color in the famous group 17 during its March return, though the writer had noted color on first appearance also. Mr. Rosebrugh, of Waterbury, reported color in spots also seen colored by the writer. It is noteworthy that meridian passage of colored groups was followed by violent magnetic disturbances and bright aurorae, some even visible at Baltimore. Dr. Moore began active observing in August, and in December confirmed color in a spot previously noted by the writer. This confirmation was obtained through the 12.5-inch reflector.

It may be mentioned that the program is so arranged as to preclude the possibility of suggestion. Color reports are submitted to the writer at the end of each month, when a résumé is made of them and mailed to the various observers. Thus no observer knows what the other has seen until the event is long past. In view of the striking confirmations obtained in this way considerable confidence is felt in the objectivity of the observations.

In conclusion, the writer would like to express his gratitude to Dr. Donald H. Menzel, of the Harvard College Observatory, who kindly reviewed this paper and offered many valuable suggestions.

300 North Eutaw Street, Baltimore 1, Maryland.

The Planets in March, 1948

By RAYMOND H. WILSON, JR.

Note: The time employed is Central Standard Time unless otherwise indicated. The phenomena have been chosen and described for the North American continent, and especially for the United States. The basic data have been taken principally from the *American Ephemeris and Nautical Almanac*.

Sun. This is the month during which the sun's northward motion is most rapid. At the end of the month it will stand 4 degrees north of the equator. The exact time of crossing the equator, called the vernal equinox, is March 20 at 11 A.M.

Moon. The phases of the moon will occur as follows:

Last Quarter	March	2	11 A.M.
New Moon		10	3 P.M.
First Quarter		18	6 A.M.
Full Moon		24	9 P.M.

The moon is nearest to the earth on March 23.

Again this month there will be an occultation of a star δ Scorpii just after the star has risen at 11 P.M. Eastern Standard Time on March 28. Therefore, the phenomenon will be visible only in the eastern seaboard states.

Evening and Morning Stars. Venus will appear extremely brilliant in the western evening sky, while Saturn and Mars will be in the east. Jupiter may be seen in the southeastern sky after 3 A.M.

Mercury. On March 17 Mercury will be 28 degrees west of the sun in the morning twilight, but a southern declination of 13 degrees will be unfavorable to its visibility for northern observers. On the morning of March 8 it will be 6 degrees north of the waning moon.

Venus. This most conspicuous planet will remain above the horizon in the evening until nearly 10 P.M. It will be just west of the Pleiades.

Mars. This planet, just past opposition, continues as a highly conspicuous red star, just northwest of Regulus in the evening sky. It will move westward a few degrees during the month. In the early evening of March 21 it will be only a degree south of the moon.

Jupiter. On March 18 this planet will be 90 degrees west of the sun along the ecliptic. It thus will be fairly conspicuous to early morning observers, although at a low southern declination.

Saturn. This will be another planet favorably situated in the eastern evening sky. It will be some 5 degrees west of Mars, moving westward more slowly than the red planet.

Uranus. Uranus will be moving very slowly northeastward at a position 3 degrees northwest of ζ Tauri.

Neptune. Neptune will be moving slowly northwestward at a position 2 degrees south of γ Virginis.

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Asteroid Notes

By HUGH S. RICE

VESTA is still observable with small instruments, and at this time is changing from retrograde to forward motion in the middle of Gemini. It has been seen from New York by several observers. The computed ephemeris gives a maximum photo-magnitude of 7.8 for January 10. Allowing an empirical correction of —1.2 magnitudes from photo to visual magnitude, this would mean a visual magnitude of 6.6 at the brightest. By means of exact magnitudes of comparison stars, observers Edward G. Oravec and Kenneth Weitzenhoffer found magnitude 6.3 on January 11 and 14. The discrepancy is explained by a graph of the predicted magnitudes. The graph shows the real maximum comes a little later than January 10—an illustration of the fact that inspection alone, of a series of values often does not determine a maximum or minimum. Moreover the opposition date is January 14.

27 EUTERPE is in Leo, and at its brightest (which we believe is about 9.4

magnitude) at the beginning of March. 6 HEBE is in western Virgo, and at its brightest (which we believe is about 9.4 magnitude) shortly before the middle of March.

Following are ephemerides which are part of the asteroid program of the Yale University Observatory.

ASTEROID EPHEMERIDES
For 0^h U.T. Equinox 1948

4 VESTA

1948	α	δ
	h m	° ′
Feb. 19	7 4.8	+25 45
24	7 3.3	+25 54
29	7 2.6	+26 2
Mar. 5	7 2.8	+26 7
10	7 3.8	+26 11
15	7 5.5	+26 13
20	7 7.8	+26 13

27 EUTERPE

1948	α	δ
	h m	° ′
Feb. 19	11 3.7	+ 8 50
24	10 59.0	+ 9 22
29	10 54.2	+ 9 54
Mar. 5	10 49.4	+10 25
10	10 44.7	+10 54
15	10 40.3	+11 19

6 HEBE

1948	α	δ
	h m	° ′
Feb. 19	12 23.1	+ 9 58
24	12 20.5	+10 47
29	12 17.3	+11 38
Mar. 5	12 13.7	+12 29
10	12 9.8	+13 20
15	12 5.6	+14 8

Hayden Planetarium, American Museum of Natural History,
New York, N. Y., January 20, 1948.

Occultations for March, 1948

(Taken from the *American Ephemeris*)

Date 1948	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION a	IMMERSION b	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION a	EMERSION b	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
Mar. 1	172 B.Libr	5.9	10 50.2	−3.4	+1.3	47	11 22.0	−0.6	−3.4	3
19	BD+27°1122	6.5	2 32.9	−1.3	−0.7	77	3 36.3	−0.4	−2.0	298
20	28 Canc	6.1	22 20.4	−1.0	+2.4	62	23 17.4	−1.6	−1.4	319
21	υ^1 Canc	5.7	0 3.6	..	..	35	0 29.4	..	..	357
23	46 Leon	5.7	0 8.0	−1.1	+0.2	116	1 18.3	−1.4	−0.3	298
28	25 Libr	6.0	9 30.6	−1.2	−2.1	162	10 22.3	−1.6	−0.7	246
29	δ Scor	2.5	4 20.3	−0.9	+1.5	86	5 17.6	−0.3	−0.4	328
29	56 B.Scor	5.9	8 13.9	−1.1	−1.1	152	9 19.2	−2.3	−0.1	254
29	27 G.Scor	5.8	9 39.0	−1.8	−1.0	115	11 0.0	−1.5	−1.2	280
31	10 G.Sgtr	5.8	7 44.7	−0.1	−1.1	160	8 29.3	−2.9	+2.2	226
OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′										
Mar. 1	172 B.Libr	5.9	9 55.6	−2.2	+0.6	90	11 8.6	−1.3	−1.3	328
19	BD+27°1122	6.5	2 6.6	−1.8	−1.1	102	3 23.5	−1.4	−0.9	269
20	υ^1 Canc	5.7	23 27.4	−0.8	+3.5	47	0 11.1	−1.7	−2.4	333
22	46 Leon	5.7	23 51.5	−0.6	+0.1	125	0 54.0	−1.0	+0.8	280
29	27 G.Scor	5.8	9 9.5	−1.4	−0.8	137	10 27.4	−2.2	−0.4	270
30	118 B.Ophi	6.2	10 36.2	−2.3	+0.1	87	12 2.0	−2.0	−1.2	298

Date 1948	Star	Mag.	IMMERSION Greenwich C.T. h m	IMMERSION a m	IMMERSION b m	IMMERSION Angle E from N °	EMERSION Greenwich C.T. h m	EMERSION a m	EMERSION b m	EMERSION Angle E from N °
Occultations Visible in Longitude +98° 0′, Latitude +30° 0′										
Mar. 1	172 B.Libr	5.9	9 42.0	—1.4	—0.5	126	11 7.3	—2.1	—0.7	298
19	BD+27°1122	6.5	2 17.3	..	..	150	3 5.7	—3.0	+2.8	220
20	*c* Gemi	5.4	7 38.8	—0.8	—0.1	62	8 17.2	+0.8	—2.2	332
20	υ^1 Canc	5.7	23 0.5	—0.8	+1.2	82	0 9.5	—1.7	—0.1	293
20	υ^2 Canc	6.4	23 45.6	—1.1	+1.7	72	0 52.6	—2.0	—1.1	308
22	46 Leon	5.7	23 56.1	—0.9	—2.6	167	0 30.4	—0.6	+3.3	235
29	27 G.Scor	5.8	9 19.1	+0.2	—3.2	174	10 3.9	—4.1	+1.8	238
30	118 B.Ophi	6.2	10 20.7	—2.3	—0.4	112	11 55.3	—2.6	—0.6	280
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′										
Mar. 1	172 B.Libr	5.9	9 23.6	0.0	—0.9	152	10 24.5	—1.8	+1.0	270
15	*o* Arie	5.8	3 55.2	—0.4	—3.5	125	4 33.2	—0.8	+2.1	196
19	BD+27°1122	6.5	1 9.5	—2.3	—0.6	114	2 23.4	—2.1	+1.8	237
20	*c* Gemi	5.4	7 13.4	—1.2	—0.9	86	8 12.8	—0.1	—2.0	311
27	λ Virg	4.6	6 44.5	—1.5	+1.3	87	7 38.7	—0.2	—1.6	344
30	118 B.Ophi	6.2	9 46.7	—0.7	—0.2	134	11 1.2	—2.2	+0.7	269

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

In these Notes will be given a synopsis of reports for July and August which did not reach me in time to be included in the Notes for October, 1947. The tables contain the usual data. Perhaps attention should be called to the fact that in the column headed R, the rate given is *uncorrected*. To secure the corrected rate, one must divide by F. Even then no allowance is made for time taken by observers to plot or record meteors. Hence those who count only, designated by c in the last column, would always have a higher hourly rate than others who plot or describe the meteors seen. It is safe to assume that our average observer takes at least one minute to plot or (and) record each meteor he sees. When I was in the best practice, it used to take me 40 seconds; I do not think it can be done in appreciably less time by anyone.

We have the pleasure of having received a good report of work done in Belgium. This is sent by W. de Kort; he observed near Antwerp. He is connected with the Société Astronomique d'Anvers. Until his meteors are replotted

from the coordinates given in his table, the possible radiants cannot be derived so only his data are given.

In my second table will be found AMS radiants No. 1969 to 1999 inclusive and 2401. The discontinuity of the numbers is due to our desire to fill up unused numbers. With this list the numbers up to 2228 have mostly appeared. From 2229 to at least 2309, the numbers are reserved for a catalogue of fireballs, descriptions of which have appeared from time to time in these Notes but whose radiants were unknown or very uncertain. The radiants herein given are, as will be seen, largely derived from the maps of a very few members. This is due to the fact that many of our members plot so seldom that they are never really in practice and that they work too few consecutive hours once they decide to observe. Hence it cannot be expected that radiants of value can be derived from their maps. This does not imply that their records are not valuable from every other standpoint and are not used, but to plot well one must *keep in practice*. At best it is not an easy thing to do with accuracy as I can testify from nearly half a century of practical experience. Since it is not wise to derive rough or uncertain radiants, when obviously they are so, I refrain from publishing them. Incidentally, it is not an arbitrary decision on my part: there are certain checks which can be applied to meteor plots submitted to me. Not until the plots come up to a certain standard am I willing to publish radiants based thereon. However, the present table contains only a part of the radiants already derived, and many others remain to be worked up. Hence many observers probably will find their radiants for 1947 in next month's Notes, and need not be apprehensive that they will not appear in print.

It will be noted that Tommy Scott of Nauvoo, Alabama, leads everyone in number of radiants. This is due to his having observed 1400 meteors in 1947, nearly all plotted and fully recorded, working on more than 48 nights. Also he rarely spent less than two consecutive hours observing, often up to four or five. He also has been studying the subject, and these things, added to his living where atmospheric conditions are good on the whole, have made his contributions to the AMS one of the outstanding ones since the Society began to function. While it is impossible for persons in or very near large cities to compete on equal terms, still we have many who live in small places or the country. The excellent results obtained by Scott should inspire others to greater exertions and better work.

In the radiant table Nos. 1969-1986 refer to thePerseids. Nos. 1971, 1980, and 1984 from Scott's maps refer to what seems to be a minor branch of the main stream. Indications of these are found on the maps of many others but not clearly enough defined to justify deriving a radiant. It will also be seen that we have violated our usual rule by publishing several radiants based on but three meteors. However, it will be noted that Nos. 1987 and 1988 confirm one another; also 1990 and the unnumbered one which follows; also 1933 confirms 1994. Nos. 1989, 1991, and 1992, though unconfirmed by other radiants of this year, still have other meteors on his or the maps of others for the same date, which make their existence probable.

Will all members, who have not paid their dues for 1948, please send them on without forcing me to spend time and AMS postage sending them a reminder? As I have no day assistant to help me in such useless correspondence, this aid would be much appreciated. F. O. Reprint No. 69, my second paper on long-endur-

ing meteor trains, is now ready for distribution. It and No. 68, which will cover Meteor Notes for 1947, will both be sent to all members who have paid their dues, as soon as No. 68 arrives. This will probably be about March 1. Astronomers and others, who may not be on any of our mailing lists but are specially interested in meteor train phenomena, may obtain a copy at once by writing me.

Station, Observer and Time	Date 1947	Began	Ended	Min.	Met.	Fac.	R	Ob.	C
Connecticut									
New Britain—E.S.T.									
Posten, Harry	Aug. 10	11:30	13:43	133	51	0.8	23	1	P
District of Columbia—E.S.T.									
National Capital									
Astronomers	Aug. 8	9:10	9:40	30	6	0.3	..	3	C
" "	10	8:30	9:00	30	4	0.1	..	2	C
" "		9:30	10:00	30	3	0.3	..	1	C
" "	11	8:15	8:45	30	8	0.3	..	5	C
" "		8:45	9:15	30	16	0.3	..	6	C
" "		9:15	9:30	15	2	0.1	..	7	C
" "	12				10	0.3	..	3	C
Iowa									
Sibley—C.S.T.									
Fitzsimmons, C. S.	Aug. 10	10:10	12:15	125	15	0.6	7	1	P
" "	11	11:15	13:30	75	21	0.5	17	1	C
" "	12	10:30	11:15	45	5	0.3	..	1	C
" "	13	9:15	11:15	120	30	1.0	15	1	C
" "		13:05	14:05	60	32	1.0	32	1	C
Kentucky									
Columbia—C.S.T.									
Bybee, G. F.	July 2	8:00	9:50	110	10	0.5	5	1	P
" "	4	8:15	9:15	60	10	0.8	10	1	P
" "	11	8:00	9:00	60	7	0.7	7	1	P
" "	12	8:00	9:15	75	7	0.7	6	1	P
" "	18	10:00	11:00	60	8	0.6	8	1	P
" "	22	8:30	11:30	180	12	0.7	4	1	P
" "	Aug. 2	8:00	9:00	60	7	0.7	7	1	P
" "	3	8:00	9:00	60	12	0.8	12	1	P
" "	4	8:00	10:00	120	16	0.8	8	1	P
" "	5	8:00	9:20	80	12	0.7	9	1	P
" "	8	8:00	9:15	75	20	0.8	16	1	P
" "	9	8:00	12:00	240	28	0.8	7	1	P
" "	11	8:15	11:20	185	51	0.7	17	1	P
" "	12	10:00	12:00	120	24	0.8	12	1	P
" "		12:00	14:15	135	16	...	7	1	P
Maine									
Cape Elizabeth—E.S.T.									
Dole, R. M.	Aug. 11	11:30	14:30	180	58	...	19	1	P
" "	12	11:30	13:30	120	18	0.5	9	1	P
Saco									
Graves, Mrs. P. E.	11	9:45	11:45	120	14	...	7	1	C
" "	12	11:30	13:45	135	36	...	16	1	C
Maryland									
North East—E.S.T.									
Hukill, R. M.	July 28	14:00	16:00	120	21	1.0	10	1	P
Blossom Point—E.S.T.									
Johnson, L. T.	Aug. 14	9:10	13:37	240	40	0.9	10	1	P
Massachusetts									
Allerton—E.S.T.									
McConarty, Miss E. M.	Aug. 10	11:15	12:15	60	21	...	..	2	C

Station, Observer and Time	Date 1947	Began	Ended	Min.	Met.	Fac.	R	Ob.	C
New Hampshire									
Strafford—E.S.T.									
Dupee, Mrs. D. S.	Aug. 11	10:15	13:15	180	52	...	17	1	P
" "	12	10:15	11:45	90	29	1.0	19	1	P
" "	12	12:15	14:15	120	49	1.0	24	1	P
" "	13	9:30	11:00	90	10	1.0	7	1	P
" "		11:00	12:30	90	21	1.0	14	1	P
" "		13:00	15:00	120	37	1.0	18	1	P
Munsonville									
Rayna, G.	Aug. 12	11:35	13:35	120	23	0.4	12	1	D
New Mexico									
Albuquerque—M.S.T.									
Lane, F. O.	Aug. 9	14:14	16:20	126	21	0.4	10	1	D
New York									
Bronx—Crotona Observers—E.S.T.									
Kohn, K. W.	Aug. 10	8:50	13:00	235	13	0.3	..	1	P
" "	11	10:06	15:06	300	54	0.6	11	1	P
Rayna, G.	Aug. 10	10:45	12:00	75	9	0.9	7	1	P
East Aurora—E.S.T.									
Dow, J. P.	Aug. 10	10:15	12:25	120	20	...	10	1	P
" "	11	8:15	10:54	...	3	0.2	..	1	P
" "		14:04	16:45	161	13	0.5	5	1	P
Missert, R.	Aug. 10	10:15	12:25	130	23	1.0	11	1	P
" "	11	8:20	16:45	...	16	0.5	..	1	P
W. Yonkers—E.S.T.									
Epstein, E. S.	July 28	12:25	13:39	74	6	0.6	5	1	P
" "	Aug. 11	10:03	15:48	345	69	0.8	12	1	P
Yonkers—E.S.T.									
Stowe, B.	July 26	12:25	13:55	90	8	0.8	5	1	D
" "	Aug. 5	11:10	12:38	88	11	0.8	7	1	D
" "	10	9:55	11:55	120	16	0.7	8	1	P
" "	11	10:05	15:47	342	82	0.8	14	1	P
Munsonville—E.S.T.									
Kissel, S.	Aug. 10	10:45	11:45	60	22	1.0	22	1	C
" "		11:45	12:45	60	22	1.0	22	1	C
Chabot, H. L.	July 9	10:30	11:42	...	5	0.6	..	1	P
" "	29	9:40	11:40	120	10	0.3	5	1	P
" "	Aug. 5	9:10	10:10	60	5	0.5	5	1	P
" "	7	9:55	11:25	90	12	1.0	8	1	P
" "	10	9:45	11:45	120	24	1.0	12	1	P
" "	12	10:35	13:05	120	28	0.4	14	1	P
Levinson Manor—E.S.T.									
Teleporos, A.	Aug. 10	11:09	12:20	71	7	0.8	6	1	D
" "	11	11:05	12:03	58	11	0.8	11	1	D
" "	12	11:06	13:00	114	92	1.0	48	1	D
Bronx—E.S.T.									
Weitzenhoffer, K.	July 25	13:15	15:00	105	5	0.5	3	1	D
" "	26	12:30	14:00	90	6	0.6	4	1	D
" "	Aug. 10	12:00	14:30	150	10	0.7	4	1	D
" "	11	10:05	15:45	340	69	0.8	12	1	D
Nebraska									
Kearney									
Martin, Miss P.	Aug. 11	9:00	11:00	120	20	...	10	1	C
North Carolina									
Fort Bragg									
Schilling, J. H.	July 19	10:00	11:45	...	10	1.0	..	1	P
" "	Aug. 10	16:00	17:30	...	4	0.9	..	1	P

Station, Observer and Time	Date 1947	Began	Ended	Min.	Met.	Fac.	R	Ob.	C
Oklahoma									
Norman—C.S.T.									
Whitney, Prof. B. S.	Aug. 12	9:40	10:40	60	10	1.0	10	1	c
" "		10:43	12:08	85	43	1.0	30	1	c
Boles, F. W.	Aug. 12	10:05	11:05	60	17	1.0	17	1	c
" "		11:10	12:10	60	35	1.0	35	1	c
Washington									
Pasco—P.S.T.									
Oberst, W.	Aug. 12	9:40	10:10	30	14	1.0	28	1	c
" "		15:20	15:50	30	30	0.7	60	1	c
Port Blakely									
Muszalski, R. J.	Aug. 11	11:30	14:35	185	33	1.0	11	1	p
Michigan									
Holland									
Kingman, D.	Aug. 5			90	51	1.0	34	1	c
" "	9			60	67	1.0	67	1	c
" "	12	12:		60	97	1.0	97	1	c
Belgium									
Société d'Astronomique d'Anvers—G.M.T.									
Reported through W. de Kort									
" "	July 28	9:15	10:45	90	6	0.6	4	..	p
" "	Aug. 1	9:15	9:50	35	2	0.5	4	..	p
" "	8	8:55	10:35	95	6	0.8	4	..	p
" "	9	9:10	10:58	108	20	1.0	11	..	p
" "	10	8:45	10:55	130	21	1.0	10	..	p
" "	11	8:55	11:11	136	50	1.0	22	..	p
" "	12	9:10	11:10	120	32	0.9	16	..	p
" "	13	9:25	10:35	70	18	0.9	15	..	p
" "	14	8:45	10:45	120	26	0.9	13	..	p
" "	15	9:30	10:10	40	3	0.7	4	..	p

AMS No.	Date 1947	R.A.	Decl.	Meteors	Accuracy	Observer
1969	Aug. 7.7	43°	+55	6	fair	Scott, T.
1970	9.7	41.5	55.5	4	good	Scott, T.
1971	9.7	55	56	3	good	Scott, T.
1972	9.7	42.5	56	8	good	Siekman, W.
1973	10.7	42	55	20+	fair	Posten, H.
1974	10.9	43	55	4	good	Schilling, J. H.
1975	10.8	44	57	3-6	fair	Wright, M. G.
1976	11.7	48	53	8-9	fair	Burns, H. A.
1977	11.7	45	54	39	good	Dole, R. M.
1978	11.7	42	55	9+	good	Epstein, E. S.
1979	11.8	43.5	54	37	good	Scott, T.
1980	11.8	57	54.5	8	poor	Scott, T.
1981	11.8	46	56.5	20	good	Siekman, W.
1982	11.8	45	56	8	f.g.	Wright, M. G.
1983	12.8	47	54	32	good	Scott, T.
1984	12.8	56	57	15	good	Scott, T.
1985	14.7	50	54	6	good	Johnson, L. T.
1986	14.7	52	53	4	poor	Scott, T.
1987	12.8	4	57	4	v.g.	Scott, T.
1988	11.8	9	58	3	good	Scott, T.
1989	13.7	240	52½	3	good	Scott, T.
1990	12.8	223	56	4	fair	Scott, T.
....	9.7	267	55	2	....	Scott, T.
1991	11.8	288	55	3	good	Scott, T.
1992	14.7	316	50.5	3	good	Scott, T.

AMS No.	Date 1947	R.A.	Decl.	Meteors	Accuracy	Observer
1993	9.7	296	63	3	fair	Siekman, W.
1994	11.8	303	63	3	good	Siekman, W.
1995	11.7	343	66	11	good	Dole, R. M.
1996	July 28.3	3	24	5-6	f.g.	Siekman, W.
1997	28.3	334	+18	5	fair	Siekman, W.
1998	28.3	340	—19	9+	good	Siekman, W.
1999	Mar. 26.6	183	—10	8F	fair	Wood, R. J.
2401	26.6	205	+ 5	9F	fair	Wood, R. J.

1947 December 30.

Contributions of The Meteoritical Society

(Known Formerly as The Society for Research on Meteorites)

Edited by FREDERICK C. LEONARD

Department of Astronomy, University of California, Los Angeles 24

Synthetic Metallic Meteorites

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ABSTRACT

Previous successful attempts to synthesize metallic meteorites are reviewed. The author also has been successful in synthesizing an octahedrite containing 27.7% nickel. Other alloys were prepared containing various amounts of iron, nickel, and cobalt, and 2 of these showed poorly developed Widmanstätten figures. The composition, heat treatment, hardness, and, in some cases, specific gravity of these alloys are given.

A number of past attempts have been made to make synthetic meteorites, but the earliest to be successful seems to have been that of Bendicks.[1] He ignited a mixture of thermite, nickel oxide, and additional aluminum, and cooled the melt during 60 hours. It is said that a polished and etched surface showed Widmanstätten figures that were faintly visible to the naked eye. Under a microscope they were clearly visible, and were compared by Bendicks to the figures of the N'Goureyma, Jenné, Macina, French West Africa, iron (cl. = ObZG). The comparison was a good one, as the figures in each case were somewhat blurred. The analysis of the synthetic alloy gave: Fe 88.17%, Ni 11.7%, P 0.034%, and C 0.09%. A synthetic hexahedrite also was made in a similar manner. It showed no crystallization figures, but, on being broken, it exhibited good cubic cleavage. The analysis of this alloy was: Fe 93.10%, Ni 6.70%, P 0.14%, and C 0.06%.

The next successful attempt was made by Derge and Kommel.[2] Their alloy contained 27% nickel, and was cooled from 1400° C. during 14 hours, after it had been held for 2 weeks at that temperature in a hydrogen atmosphere. The resulting Widmanstätten figures were highly perfect and were visible to the naked eye. Another alloy containing 31% nickel gave a similar result.

The experiment of Smith and Young[3] is best described as partially successful. They slowly cooled an alloy containing 10% nickel from the liquid. This

showed no Widmanstätten figures, and X-ray examination showed only body-centered crystals. After annealing at 580° C., the X-rays revealed the presence of some face-centered material with an orientation similar to that in a meteorite. The amount of taenite formed was so small, however, that it never became visible, and could be detected only by X-ray analysis.

The present author tried to use Bendicks' method, modified by the use of metallic nickel-powder instead of nickel-oxide and aluminum. The melts obtained in this way usually contained aluminum and silicon; these elements made them exceedingly hard, and none contained any Widmanstätten figures; consequently, the following method was adopted. Iron and nickel powders were mixed with a small amount of cobalt and a little red phosphorus, and the mixture was melted in a magnesia-lined crucible in an induction furnace. As soon as the button had solidified, it was transferred to an electric muffle preheated to 1000° C. and cooled during 24 hours to room temperature. Microscopic examination showed excellent Widmanstätten figures, closely resembling those obtained by Derge *et al.*, except that they were considerably smaller. The composition of the alloy was very similar to that of Derge's. This fact seems to indicate that the size of the figures depends upon not only the composition, but also, to some extent, the heat treatment; thus, Derge's alloy was kept for 2 weeks at a high temperature, and the figures were visible to the naked eye, while my alloy was heated for only about a day, and the figures were microscopic. Because of this success, several other alloys were prepared in a similar manner, with the difference that the buttons were allowed to cool rapidly to room temperature and were heat-treated at a later time. The crucibles were of alumina. The composition, the Rockwell hardness, and, in some cases, the specific gravity of these alloys are given in Table 1.

TABLE 1

No.	Fe	Ni	Co	Hardness	Sp. Gr.	Structure
1	79.40%	17.16%	0.92%	*B*-85	...	Very poor Widmanstätten figs.
2	70.48	27.69	0.86	*B*-78	...	Very good, small Widmanstätten figs.
3	73.10	14.74	10.86	*B*-98	7.958	Martensite near surface; Widmanstätten figs. possibly in center
4	73.20	n.d.	25.20	*B*-82	7.939	Homogeneous solid solution
5	1.40	18.42	79.10	*B*-72	8.536	Unidentified; very tough

The heat treatments were as follows:

1: Reheated to 900-1000° C. for 22 hours, then cooled to room temperature during 7 hours.

2: Cooled from 1000° C. to room temperature during 24 hours.

3: Reheated to 900° C. for 2 hours, and cooled to room temperature during 16 hours.

4: Reheated to 595° C. for 2 hours, and cooled to room temperature during 24 hours. Ghosts of the original γ-structure were visible without etching. An additional treatment of 30 minutes at 855° C., then of 1 hour at 595° C., followed by cooling to room temperature during 12 hours, removed the ghosts, but the resulting structure was homogeneous.

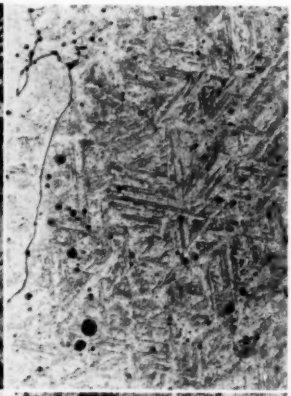
5: Identical with No. 3.

The compositions are the results of actual analyses by F. G. Hawley. The differences in the sums from 100% are due probably to oxide, insoluble matter,

and undetermined minor components. The specific gravities were determined by the pycnometric method, and are to be regarded as minima, because the alloys contained blebs of oxide.

FIGURE 1

FIGURE 2



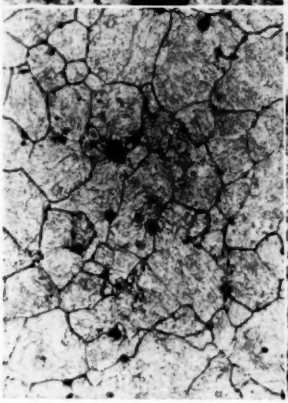


FIGURE 3

FIGURE 4

FIGS. 1-4

PHOTOMICROGRAPHS OF POLISHED AND ETCHED SECTIONS OF 4 SYNTHETIC METALLIC METEORITES (× 125)

(1) Fe 79.40%
Ni 17.16
Co 0.92

Picral Etch

(2) Fe 70.48%
Ni 27.69
Co 0.86

Picral Etch

(3) Fe 73.10%
Ni 14.74
Co 10.86

Picral Etch

(4) Fe 1.40%
Ni 18.42
Co 79.10

HNO_3 (10%) Etch

Composition No. 1 has a structure so irregular that it is not certainly Widmanstätten, but a careful study of numerous areas seems to indicate that it *is* a poorly developed Widmanstätten structure.

Composition No. 2 has already been described. It is difficult to understand why compositions similar to No. 1 show relatively poor, or no, Widmanstätten figures, altho these compositions are close to the average composition of all siderites; on the other hand, natural meteorites with 27% nickel are usually, if not always, nickel-rich ataxites, whereas perfect artificial structures have not been obtained with much less than this amount of nickel.

Composition No. 3 was made to see whether some of the nickel could be replaced by cobalt and Widmanstätten figures could still be obtained. The nickel alone is insufficient, but the nickel plus the cobalt totals 25.6%, or nearly as much as in composition No. 2. The structure at the edges of the button is undoubtedly martensitic. The thickness of this layer is about 2 mm., but it merges rather rapidly into a somewhat similar structure, in which the needles are much coarser and do not differ in color from the interstitial metal. In the martensitic areas, the needles etch black with picral. The central area also requires very long etching for its development, and resembles both martensite and a Widmanstätten structure.

Composition No. 4 gave no structure, altho it was given 2 heat treatments, and several different etchants were tried. This result was probably to be expected, in consideration of the iron-cobalt diagram.

Composition No. 5 was made to see whether nickel and cobalt were capable of giving a Widmanstätten-like structure. This alloy was not particularly hard, but it was so tough that it could not be cut by a hacksaw; all the others cut relatively easily; it was also extraordinarily resistant to etchants. Picral revealed only a polygonal grain-structure. 2% nitric acid in ethyl alcohol slowly revealed traces of structure, but fairly long etching with 10% nitric acid in water was required to give a proper etch. The structure consists of somewhat wavy bands, often pointed at the ends, and pointing usually in only 1 direction in any given grain, but pointing occasionally in 2 or 3 directions. Many grains do not show these bands, while some grains show twinning bands. From a consideration of the nickel-cobalt diagram, it is seen that this composition lies close to the boundary between the α- and β-structures; it may be that the bulk of the alloy has the closely packed, hexagonal β-structure; this structure is suggested also by the high density and the brittleness of the alloy. The streaks in it may be the face-centered, cubic α-structure; in that case, the alloy would lie in the very narrow region between the 2 forenamed structures. The exact position of this region is imperfectly known, but if the foregoing interpretation is correct, it may lie closer to 100% cobalt than previously supposed.

My thanks are due to Mrs. Virginia Johnson for valuable assistance in the preparation of these alloys for microscopic examination.

REFERENCES

[1] C. Bendicks, *Metallographic Researches*, pp. 109-18, McGraw-Hill, 1926.
[2] G. Derge and A. R. Kommel, *Am. Jour. Sci.* (5), **34**, 203-14, 1937; G. Derge, *C.S.R.M.*, **2**, 147-54; *P. A.*, **47**, 558-66, 1939.
[3] S. W. J. Smith and J. Young, *Nature*, **143**, 348-50, 1939.

Abstracts of Two Papers Read at the Ninth Meeting of the Society, 1946 September

LINCOLN LA PAZ
Institute of Meteoritics, University of New Mexico, Albuquerque

CONTRATERRENE METEORITES

In 1941, the writer called attention to several anomalous craters and silica-glass deposits, apparently of meteoritic origin, with which no meteorites are known to be associated, and suggested that these craters and deposits were produced by the impact of contraterrene meteorites with the surface of the Earth. This paper presents additional evidence supporting the hypothesis that contraterrene meteorites exist. In part, the evidence is derived from the theoretical considerations of Dirac (1934) and of Klein (1945), and, in part, from experimental work conducted at the University of New Mexico in the years 1943-6.

METEORITE COLLECTORS EMPLOYING PORTABLE 4900-GAUSS PERMANENT MAGNETS

Both permanent magnets and electromagnets have long been used as a means of collecting specimens of meteoritic iron on or near the surface of the ground. The permanent magnets hitherto available, while a great solace to the aching back, have been too weak to collect, except by accident, irons not first spotted by the eye. The electromagnets can be made powerful enough to search a strip a few feet wide to a depth of an inch or so, but they require extremely cumbersome auxiliary power sources. This paper describes a new type of permanent magnet, developed for military purposes, that combines portability with a remarkably intense magnetic field; its performance compares favorably with that of electromagnets energized by gasoline-driven D.C. generators.

A Comment on Jaggar's "Origin and Development of Craters" (1947)

The following comment is on "Origin and Development of Craters," by T. A. Jaggar, *Geological Society of America, Memoir 21*, 508 + xvii pp., 73 pl., August 15, 1947. This monumental work, recording observations made over the past 40 years by Dr. Jaggar and his assistants, on eruptions of the Hawaiian volcanos, Kilauea and Mauna Loa, with an extensive section (70 pages) on "Principles of Crater Evolution," is a meticulous and complete historical account. It is a noteworthy and amazing fact, however, that a work of this title and scope, in a foremost modern geological series, contains *not a single line* on the origin of terrestrial *meteorite* craters!

ROBERT W. WEBB
Department of Geology, University of California, Los Angeles,
1947 December 9

On Naming Meteoric Showers

In a letter to the editor of *Sky & Telescope* (7, No. 2, p. 30, Dec., 1947), Mr. Paul Stevens of 2322 Westfall Road, Rochester 10, New York, writes as follows:

"I wish also to call attention to the inconsistency of nomenclature found in

Sky & Telescope and in POPULAR ASTRONOMY when referring to the meteor shower of October 9/10, 1946, as the Giacobinids. It is suggested that they be called the Draconids, with the prefixing of the Greek letter [in this case, probably ν] of the star nearest the radiant (as in the δ Aquarids), if there is a previous shower with that designation. If this is not done, then the η Aquarids and the Orionids should be called the Halleids. Besides, the name Giacobinids is not fair to Mr. Zinner, whose name also is associated with the comet from which those meteors emanate."

Mr. Stevens' suggestion is good as far as it goes, but it should go farther. Meteoric showers ought to be named always for the constellations in which their radiants are located—never for comets and people. If the radiants of 2 or more showers are contained within the boundaries of a single constellation, then all such showers should be named for the months (or dates) in (or on) which they occur, as well as for their common constellation—as, *e.g.*, the May (or May (η)) Aquarids. The so-called Giacobinids, even tho they are associated with the Giacobini-Zinner comet, ought by all means to be termed the Draconids—or, better, the October Draconids—not the Giacobinids. If 2 or more showers radiate from the same constellation in the same month (either in the same year or in different years), then the specific date (or dates) of each such shower should be used in connection with its constellational desgination—as, *e.g.*, the October 9 Draconids. F.C.L.

President of the Society: ARTHUR S. KING, Mount Wilson Observatory, Pasadena 4, California

Secretary of the Society: OSCAR E. MONNIG, 1010 Morningside Drive, Fort Worth 3, Texas

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VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

SS Cygni in 1947: The special variable SS Cygni, 213843, was assiduously observed during 1947, when more than 1100 estimates were secured on 290 nights, the light curve having been almost continuously followed throughout the year.

A tabulation of the daily means from J.D. 2432150 to 2550 follows, with the number of estimates used in each mean. The second table contains the data concerning each maximum in the same form as in previous years; the classification of the maximum is based on the mean light curves derived from the discussion of the observations made over the years 1896 to 1933, and published in *Harvard Annals*, Vol. 90, No. 3.

The mean cycle deduced from the maxima of 1947 is 54 days, as compared with a mean value of 51.6 days derived from the 362 maxima observed during the 51 years that the star has been under observation. The light curve for 400 days, J.D. 2432150 to 2550, is illustrated in the figure.

TABLE I

DAILY MEANS

213843 SS Cygni J.D. 2432150—2549

243	—0—	—1—	—2—	—3—	—4—	—5—	—6—	—7—	—8—	—9—
2150	9 8.60	6 8.82	11 8.86	8 9.14	11 9.65	6 9.72	5 10.48	6 10.78	2 11.25	2 11.80
60	1 11.9	1 12.0	1 12.0	.....	.....	1 11.7	2 12.00	2 11.95	5 11.90	4 12.05
70	3 11.73	2 12.0	3 11.93	5 11.94	3 11.93	.....	1 12.1	.....	5 11.92	2 11.95
80	2 11.80	2 11.95	2 11.90	1 12.0	2 11.90	3 11.97	.....	.....	.....	2 11.85
90	1 11.9	1 12.0	1 12.0	.....	3 11.87	4 11.85	3 11.90	3 11.97	4 11.90	1 12.1
2200	2 11.90	2 11.95	2 11.90	2 12.00	2 11.90	1 11.6	2 11.85	4 11.95	2 11.40	.....
10	2 10.15	8 9.81	2 9.45	5 9.34	6 9.12	6 9.02	2 8.80	8 8.50	8 8.51	5 8.44
20	1 8.8	.....	3 8.63	1 9.2	2 8.85	.....	.....	2 10.10	2 10.35	3 11.03
30	3 11.53	2 11.75	.....	.....	1 11.8	2 11.85	.....	1 11.9	.....	1 11.9
40	.....	.....	.....	1 11.8	.....	2 11.85	.....	1 12.0	.....	.....
50	.....	.....	.....	1 11.8	.....	.....	1 12.0	.....	.....	1 11.9
60	.....	1 11.8	.....	1 11.7	.....	.....	.....	1 11.8	.....	.....
70	1 10.9	1 10.9	2 10.25	1 9.5	4 9.72	.....	3 9.73	2 9.80	.....	1 10.7
80	.....	.....	.....	.....	.....	.....	1 11.8	1 12.0	1 11.8	2 11.85
90	1 11.9	1 11.9	1 12.0	.....	.....	.....	1 12.0	1 11.8	2 12.00	.....
2300	.....	1 11.8	1 11.9	2 11.00	1 8.8	2 8.45	1 8.4	.....	1 8.6	.....
10	.....	.....	.....	.....	1 11.4	4 11.78	.....	.....	.....	2 12.00
20	2 12.00	2 12.00	.....	1 12.0	1 12.0	.....	.....	4 11.95	.....	2 11.95
30	1 12.1	.....	3 11.93	1 12.0	2 12.10	2 11.90	2 11.95	2 12.00	1 12.0	.....
40	1 12.1	2 12.00	3 11.90	1 12.0	.....	2 11.80	3 11.87	6 11.85	2 11.95	5 11.88
50	3 12.03	2 11.95	1 12.0	5 11.86	3 11.93	2 11.95	3 11.83	7 11.87	4 11.88	2 12.00
60	2 11.95	5 12.00	1 12.0	6 12.02	4 11.95	5 11.90	3 11.83	2 11.90	1 12.2	4 11.82
70	2 11.90	3 11.90	3 12.03	3 11.93	7 11.98	6 12.02	3 11.97	2 12.00	4 12.00	7 11.96
80	5 11.90	4 11.92	3 11.93	3 11.90	4 11.95	5 11.92	7 11.94	5 11.84	7 11.90	8 11.04
90	9 9.52	14 8.76	9 8.53	8 8.34	5 8.24	8 8.18	12 8.16	7 8.44	6 8.20	8 8.38

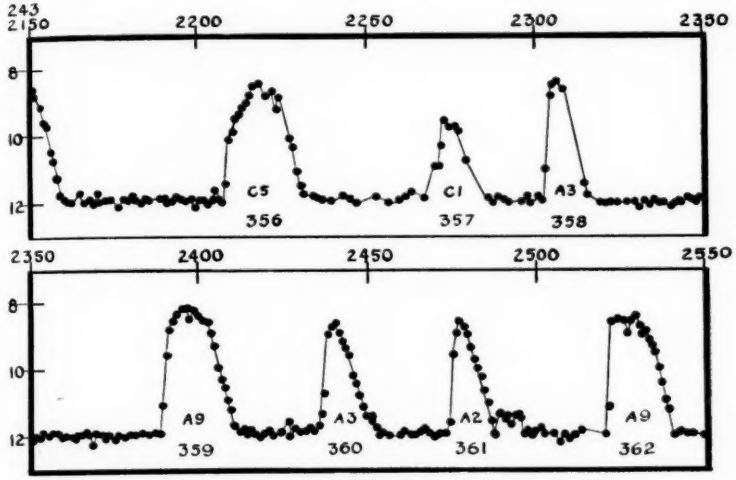
TABLE I *(Continued)*

243	—0—	—1—	—2—	—3—	—4—	—5—	—6—	—7—	—8—	—9—
2400	7 8.36	6 8.45	7 8.57	14 8.94	10 9.25	7 9.89	10 10.23	11 10.41	12 10.88	11 11.17
10	1 11.61	7 11.76	5 11.80	5 11.76	6 11.90	5 11.76	7 11.80	2 12.00	1 12.0	3 11.93
20	6 11.80	2 11.90	5 11.88	1 12.0	3 11.83	3 11.87	2 11.50	1 12.0	3 11.77	3 11.80
30	1 11.8	7 11.81	6 11.78	2 11.80	4 11.80	2 11.65	3 11.30	5 10.74	7 8.89	10 8.63
40	9 8.59	9 8.91	12 9.18	11 9.36	15 9.50	17 10.14	13 10.40	15 10.70	9 11.11	7 11.36
50	1 11.4	3 11.47	5 11.66	2 11.90	4 11.80	7 11.91	4 11.98	6 11.93	.....	2 11.95
60	1 11.8	3 11.83	5 11.88	4 11.92	6 11.89	5 11.82	4 11.78	4 11.85	2 11.85	2 12.00
70	6 11.92	5 11.96	4 11.90	8 11.90	7 11.53	12 9.45	4 8.88	2 8.50	6 8.68	13 8.91
80	12 9.26	10 9.63	7 9.91	5 10.20	1 10.6	1 11.0	3 11.47	2 11.90	.....	1 11.3
90	2 11.45	5 11.36	2 11.60	1 11.3	2 11.25	1 11.4	3 11.87	2 11.75	2 11.90	1 11.8
2500	3 11.77	1 11.7	4 11.88	5 11.88	4 11.88	2 11.90	.....	2 12.15	4 11.95	2 12.05
10	2 12.05	3 11.90	.....	1 11.8	.....	.....	.....	.....	.....	.....
20	2 11.90	2 11.10	1 8.5	4 8.50	3 8.43	.....	3 8.53	1 9.0	4 8.50	5 8.40
30	1 8.7	3 8.93	6 8.80	7 9.10	7 9.26	2 9.45	4 9.92	5 10.52	2 10.95	3 11.13
40	1 11.9	.....	2 11.85	.....	1 11.9	1 11.9	1 11.9	.....	.....	1 11.9

TABLE II

OBSERVED MAXIMA

No. Max.	Type	J.D. 10.0 I	Diff.	J.D. Max.	Diff.	J.D. 10.0 D	Diff.
355	B2	2432147	81	2432149	75	2432156	73
356	C5	2211	64	2220	71	2227	71
357	C1	2273	62	2275	55	2278	51
358	A3	2304	31	2307	32	2312	34
359	A9	2390	86	2397	90	2406	94
360	A3	2438	48	2440	43	2446	40
361	A2	2475	37	2478	38	2482	36
362	A9	2432522	47	2432528	50	2432537	55



SS CYGNI: DAILY MEANS, 1947

SS Aurigae, 060547: Three well observed maxima of SS Aurigae were noted during 1947, one of the broad type early in February, one of narrow type early in April, and one of broad type about December 12. Doubtless one or two maxima passed unobserved between June and September.

U Geminorum, 074922: Two maxima of U Geminorum were observed during the year, one of the broad type late in May, and another of the same type about October 20. A narrow type maximum was probably missed early in August.

Z Camelopardalis, 081473: This star was well observed during the year. Twelve maxima were recorded, with little chance of any having been missed. Approximate dates are as follows: January 21, February 24, March 29, April 24, May 25, June 23, July 17, August 12, September 13, October 5, November 8, and December 7—practically one a month, or a mean cycle of 29 days.

A total of 4,009 estimates was received in December, 1947, from 58 observers as follows:

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Ahnert, P.	26	65	Elias	14	24
Alder	3	5	Escalante	27	28
Ashbrook	1	6	Fernald	327	836
Bartlett	2	34	Focas	12	12
Bicknell	12	70	Greenley	35	58
Bogard	35	83	Harris	3	3
Caraioryis	5	12	Hartmann	175	197
Chassapis	81	200	Hiett	4	13
Cilley	35	100	Inman	1	1
Cragg	88	103	Kelly	14	14
Daley	2	5	Kilby	6	11
Darling	12	39	Kirchhoff	12	54
Darnell	4	16	Kitley	47	61

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Lane	1	2	Penhallow	5	11
LeVaux	15	136	Peters	1	1
Levitt	1	1	Plybon	3	9
Loden	2	12	Rosebrugh	20	129
Lopez	1	6	Sherman	10	14
Luft	6	45	Slemaker	18	22
Mary	7	7	Smith, B.	37	94
Matthews	23	34	Taboada	49	55
Meek	32	192	Tarbell	7	12
Miller	19	30	Tifft	27	51
Nadeau	23	28	Topham	18	22
Oravec	52	200	Ventar	10	40
Paletsakis	31	519	Walko	5	13
Park	1	4	Webb	15	15
Parks	28	35	Weitzenhoffer	3	16
Pearcy	25	26	—		—
Peltier	143	179	58	totals	4009

January 15, 1948.

Comet Notes

By G. VAN BIESBROECK

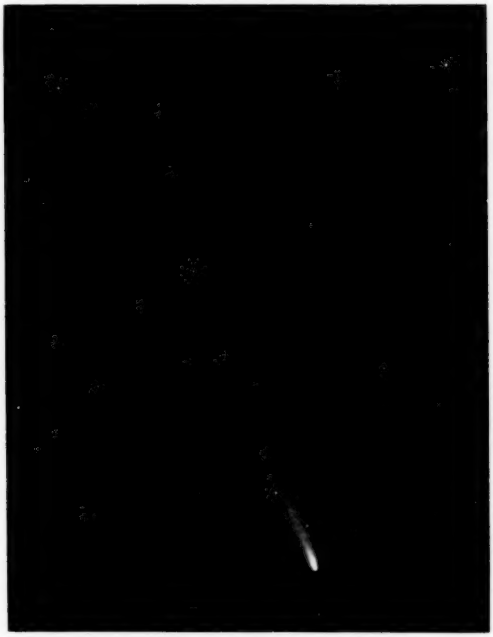
COMET 1947 *n*. A very bright unexpected comet startled the inhabitants of the southern hemisphere in the evening of December 8. This brought to 14 the number of comets discovered in 1947 which is one more than the previous record of 13 in 1932. The newcomer must have been an impressive sight with a head rivalling the brightest stars and a tail estimated to be as long as 25 degrees.

Mr. Carlos A. Etchecopar, director of the Montevideo Observatory, sent the photograph which is reproduced here. The photograph was made by E. Rondanina on December 14, with an exposure of only four minutes.

The information received from the south mostly through the daily press soon indicated that the apparition was of short duration. On the first days the comet was so close to the sun that observations were very difficult. As it moved northeastward into the evening sky visibility improved but at the same time the brightness dropped considerably. A first parabolic orbit was given by Bobone, Cordoba (Argentina), as follows:

Perihelion passage 1947 Dec.	2.592 U.T.
Node to perihelion	196°12′
Node	336 42
Inclination	138 42
Perihelion distance	0.1097 astronomical units.

This showed that the comet must have been a bright naked-eye object for a couple of weeks before discovery and even visible for northern observers. It, however, escaped detection on account of its small angular separation from the sun. On December 2, when brightest at perihelion that separation was only one degree! On our side of the equator the comet was first observed at the McDonald Observatory, Fort Davis, Texas, on December 15. Bands in the red region of the spectrum were found, which seemed to indicate the presence of ammonia in the comet. Another peculiarity was already noticed by van den Bos at Johannesburg on December 10, namely, in the head there were two distinct



COMET 1947 *n* ON DECEMBER 14, 1947

nuclei which changed considerably in relative brightness and moved apart from day to day. On December 18 the Texas observers estimated the separation as 5″; on December 20 they found 7″.5. On December 24 a plate taken by the writer at the Yerkes Observatory shows the two nuclei 12″ apart, the preceding one being best defined while the following one was more diffuse and about half a magnitude fainter. The total brightness was then reduced to magnitude 7. The comet was observable only at very low altitude over the southwestern horizon and the tail, though visible, was very faint under those conditions.

Receding from both the sun and the earth the comet will remain visible at low altitude in the evening sky for some time. During January it passes from Capricornus into Aquarius. By the end of the month it will have dropped to magnitude 13. Not until May will it move away from the vicinity of the sun; however by that time it will be very faint.

The many faint comets previously discovered and enumerated last month are still in reach of larger instruments. COMETS JONES (1946 *h*), BESTER (1946 *k*), REINMUTH 2 (1947 *j*), and PERIODIC COMET OTERMA No. 3 can be followed in the first part of the night. Both COMETS SCHWASSMANN-WACHMANN No. 1 and No. 2 can be recorded in the middle of the night, and PERIODIC COMET FAYE which remains near 12th magnitude, a good deal brighter than was predicted.

COMET HONDA (1947 *m*) remains visible only in the southern hemisphere. In the beginning of January it showed a tail 3° in length.

Towards the end of February COMET BESTER (1947 *k*) will emerge from behind the sun and appear as a naked-eye object in the morning sky. The following ephemeris should make it possible to locate the object before dawn above the eastern horizon. It should become conspicuous in March although the maximum brightness will then be past. Perihelion having occurred on February 17, the comet should be fully developed.

1947	α h m	δ ° ′	Mag.
Feb. 27	20 24.4	−14 49	2.5
Mar. 6	20 14.9	− 8 3	2.7
14	20 4.7	+ 0 59	2.9
22	19 51.6	+13 27	3.2
30	19 30.9	+30 23	3.5
Apr. 7	18 50.5	+50 26	4.0

The magnitude is computed under the assumption of a variation according to the inverse sixth power of the distance from the sun and may be quite different, but the run of the change is well indicated.

PERIODIC COMET FORBES (1929 II) comes to perihelion next fall but it may be picked up already next month as a very faint object in the constellation Virgo.

Williams Bay, Wisconsin, January 12, 1948.

General Notes

The Rittenhouse Astronomical Society, of Philadelphia, held its first meeting in the new year on January 9, 1948, in Randal Morgan Physics Laboratory, University of Pennsylvania. The installation of new officers and an address of retiring President, Mr. Arthur S. Burgess, on "An Amateur Views a Solar Eclipse" constituted the program of the meeting.

Summary of Sun-Spot Observations at Mount Holyoke College, 1947

Month	No. of Obs.	North of Equator No. of Groups	North of Equator Av. Lat.	South of Equator No. of Groups	South of Equator Av. Lat.	Av. No. Groups at one Obs.	New Groups
January	21	15	+19°8	22	−16°8	8.1	35
February	23	23	18.8	26	14.8	9.2	46
March	27	13	17.6	32	14.6	9.6	39
April	22	22	19.0	36	14.5	9.5	50
May	25	28	19.2	37	17.3	12.4	60
June	26	32	18.0	34	17.9	10.6	56
July	23	24	18.0	27	20.4	9.5	43
August	24	24	16.0	27	15.8	11.6	46
September	21	24	14.8	25	14.3	11.0	41
October	26	22	15.6	27	13.4	9.8	36
November	22	19	17.6	22	15.2	8.6	35
December	18	17	16.1	19	16.3	7.8	28
1947 Totals	278	263		334			515
1937 Totals	144	226		164			339

	1947	1937
Average number of groups at one observation	9.88	7.90
Average latitude of 233 groups north of equator (194 groups)..	+17°.34	+16°.5
Average latitude of 282 groups south of equator (145 groups)..	—16°.04	—16°.2

The indices of activity in the table indicate a sun-spot maximum in May, 1947, with a lower peak in August. In assigning letter-designations to individual groups during the year, it was necessary to go through the alphabet nearly twenty times. On twenty-two dates 16 or more groups were counted: on May 25, in the midst of the week of maximum activity, individual spots (in 16 groups) totalled 225; on August 14, 21 groups (with 165 spots) were distinguished. In contrast March 19 found a minimum number, 3 groups totalling 4 spots, in evidence.

Comparative figures are given in the table for 1937, the year of far and away the highest maximum in previous records at this Observatory. Though the records are not strictly comparable owing to the absence of observations in July and August, 1937, it would appear that 1947 has broken the previous record; likewise it disturbs an observed tendency (noted in the Wolf spot numbers) for high and low maxima to occur in alternation.

In the year just past, 17 groups were found within 5° of the solar equator, largely south of it; 13 groups exceeded ±30° in latitude. Extreme cases of high-latitude groups were observed a month apart in nearly the same solar longitude: one at +48° seen only once, at central meridian passage on May 5; the other at —42°, watched for eleven days, passing the meridian about May 31. Group-counts were found repeatedly to be large when solar longitudes 55°, 160°, 210°, 270° were approximately central on the solar disk.

Of the 515 groups on which the 1947 figures are based, 18% were seen only once, 40% not more than thrice. Nearly equal percentages (8 to 9) were observed successively on 2, 6, 7, 8, or 10 days. 39 groups were observed more than 10 times, and 2 groups 13 times in a row. Obviously the incidence of cloudy sky, along with the actual duration of the group on the earthward side of the Sun, plays a role in these percentages. When a group persists long enough to reappear at the east limb, it is reckoned as a new group.

Spot counts and records of position were made from January to June by Miss Helen Pillans, from October to December by Miss Ruth Hayner. During the summer months, the writer was assisted on occasion by Miss Elizabeth Jacoby and Miss Barbara Hubbard. The total number of individual spots counted was 18,576.

ALICE H. FARNSWORTH.

John Payson Williston Observatory, South Hadley, Mass., January, 1948.

POPULAR ASTRONOMY

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